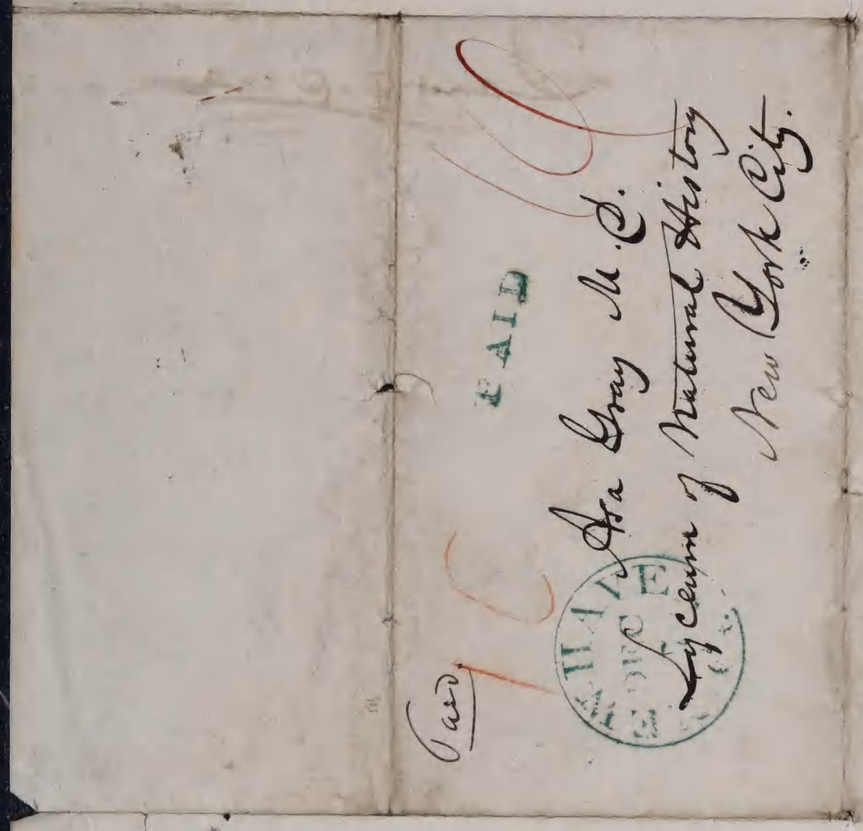


New Haven Dec 1836.
Thursday—

Friend Gray.

I perceive by a letter to Prof. S. from Dr Torrey that there is indeed some doubt of my appointment. I hardly know what means to employ to enlarge the situation, as I have no friends at Washington, and none who would probably have much influence there. Silliman has just written again, in connection with an extract from a letter I have received within a day, from Lieut. Glynn, in which he makes some remarks concerning my qualifications & which Silliman supposed might have some influence, coming as it does from a person with whom I have no acquaintance. The object of the letter was, to obtain my services as Captain's Clerk to which I should wholly object, even if I should be disappointed in the other situation - To see so many engaged in Scientific

investigations, and myself a mere looker on would be more than I would be able to endure —

Any I am highly indebted to Dr Torrey for his exertions in my behalf. and shall be exceedingly obliged to you for the endeavors you may make to secure my appointment. But the system of political favoritism has run to so high a pitch in our country that, I presume I shall have to content myself with the disappointment. How astonishingly singular it is that they make no appointments for an expedition that is to sail so soon.

Have you seen Featherstonhaugh's last geological report I beg you to read it if you wish to be amused with a tissue of absurdities and false statements, an exposition of ignorance & Geological foppery.; It will make you ashamed of your Country, or at least regret, that it should be so gulled by a pretence of Science. It is a palliation however that he is not an American, and that it is consequently in England, they must view it as the product of one of their own countrymen a member of the Royal Institution !! Do Some Geologist who can wield the pen of the Satirist I think should vindicate our country's honor, and turn the United States Geologist from the high station he holds, to the low station he should occupy and make him glad to hide his face from the eyes of our Countrymen if not from those of his own.

I shall wait patiently but anxiously for information from you as to all probabilities, and certainties if you have any to communicate. Please write soon & gratify

Your friend
in haste
James D. Dana

New Haven April 1837

Friend Gray

I intended to have given your last a more speedy reply but have been much hurried of late by my various avocations. This delay of the expedition is certainly much for my convenience, and as far as I am concerned I do not regret it. Still it would be agreeable to know with more definiteness the time of our departure; if for no other reason, to gratify inquisitive friends. I hope you will transmit the earliest news. — I have made inquiries concerning the fruit of the Centrophyllum, but without success. I do not recollect of ever having collected the plant about here; and our Doctors who interest themselves in Botany neither have the fruit collected nor can point to any locality.

If I rightly interpret the remarks of the person who carried you my last letter, the corrections for the nomenclature arrived too late for insertion. I suppose however it is of no great consequence. I should have been much gratified could I have found some

friends here, who would have carefully reviewed the whole with me previous to publication. Except in the names of the Orders, in which I received much valuable aid from Dr Tully, I was forced to depend on my own resources. I however see nothing of importance, which I am desirous of changing.

Do you know of any mineral denominated Cooperite? I received a letter from Prof. Emmons a few days since in which he inquired concerning the nature of a mineral by this name. I have seen no mention of such a species, and should be glad to have any information you may collect on the subject. Could you write soon respecting it you would much oblige yours &c. —

The following list of Geological works contains the principal desiderata, and if possible the number should be larger rather than smaller.

Lamberg on Volcannes
Humbolat's various works relating to South America
Goldfuss's Petrefaktew
De la Beche's Geology
" Views & Sections
Macculloch's System of Geology
Elie de Beaumont & Dufrénoy's Memoires pour servir à une description géologique de la France.
Cuvier's Ossemens fossiles
Boase's Primary Geology
Humbolt on the Superposition of rocks, &c.

+ Yet undoubtedly many parts admit of improvement

Brongniarts Tableau des Terrains qui composent L'Ecorce du Globe —

The Geological transactions will be exceedingly valuable, and I am highly delighted that we shall have them aboard. I wish there could also be added the Transactions of the Geological Society of France, and also some works on Comparative Anatomy. As we shall be for so long a time shut out from communication with the civilized world, we cannot have too many books on board. They will all be exhausted before we can re-provide ourselves, which will probably not be, till our return.

I am really hungry for news relative to the expeditions, and I assure you that an early reply will be very acceptable to

Your friend
James D. Dana

Asa Gray, M.D.

Do you know of any copy of Bucklands Bridgewater Treatise (English Edition) that can be obtained in Your city. The copy belonging to Prof. Silliman has been taken from my room and I am anxious to replace it.

Yours &c.

Asa Gray M. D.
Lyceum of Natural History
New York City

J.o

New Haven July 14. 1845—

Dear Gray—

You must have felt much surprise, ere this, at the apparent neglect with which your letter and all its kindness may seem to have been received. But in fact it never reached me till today. I left Philad. on the day on which it was written, and since then have been at home. By the post-mark I observe that it was advertised in Philadelphia, which probably brought it to the notice of some friend, who directed its course this way. I am much pleased, yes, delighted, with your propositions, and hope you will carry them out. I am so situated that I cannot exert much influence. I presume that Capt. W. & Dapgan favored the proposition that only 100 copies be printed by for government — and a larger number, you know, would interfere with the copy right edition. — Whether this was a motive

or not, I would not say - The objection certainly does not operate with reference to the Scientific volumes as there will be no bookstore edition, excepting the 25 copies published by Lea & Blanchard. - Letters to the Library Committee, or to others in Congress, especially if Locos - would probably produce some good result. The Library Committee have control of our affairs, and a communication from the American Academy would add essentially to the force which might be brought to bear upon them at Washington. - Lea & Blanchard objected strongly to my 25 extra Copies, and before consenting to publish their 75, obtained a promise from me that my copies should not be disposed of so as to interfere with their sales. - and in the course of their remarks said that if I should give a copy to any one at Cambridge it would prevent probably their disposing of one to Harvard !!! - It is certainly most shameful that I have not received from government even one single copy of my own work, excepting the sheets of one as it was printed, which was to be used for reference in proofreading, making out Index &c, and of course is not now as clean as a fresh one from the Binders would be.

I am much obliged to you for your favorable notice in the North American. I should be glad to be able to give you a copy of the whole work - but I am desirous, as the whole number of copies is so small, to give it as wide a distribution as possible by sending the few copies I have, abroad. - I will send you my copy in sheets in the course of a few days. It will be a fortnight probably before the last sheet printed is in my hands - Some half a dozen forms, are to be reprinted, and this is all that remains unfinished. - I have also a Copy of Mr Hale's Philological & Ethnographical volume (just completed) and if desired, I should be glad to send that also to some writer for the North American. It will be found a most creditable work, as I believe, and contains much that is highly interesting respecting the migrations of the Polynesians. - Perhaps Mr Pickering may have already received a copy through Mrs Hale.

I shall hope in future to contribute to the Academy, but at present my labors are wholly with the Expedition, except what is limited towards the Journal of Science, in the way of proof reading, Miscellanies, and some words on corals. - By the way - the Small caps you objected to in your letter to B. S. Jr. will in future be rejected - I saw no proofs of that number and knew nothing of the plan adopted, which was very different from that of the Coral books -

Let me know soon what I shall do about Hall's work — and in a week or so if desired I will give both to the Express for Cambridge. — Sincerely your friend, as ever

James D Dana

To A. Gray M. D.

Your article on Chemy of vegetation was an admirable one, so complete, satisfactory & intelligible, and I am not surprised to hear of its translation abroad. — In the Journal of Science I propose to depote a page, or less or more, to stating what is going on in the Zoological world — what memoirs have appeared or are in pipes — merely mentioning without comment, and will you contribute something of the same kind in the Botanical way? — Please remember me to Mrs Harris. I shall send him a copy of my Introduction when received — This introduction by the way makes a volume of 1400 pages. and includes every thing relating to Structure, Growth, formation & composition of corals, classification &c. every thing but the description of species. — The work nearly doubles the number of known species. —

With many good wishes, ever yours

James D Dana

New Haven — Feby 4. 1864

Dear Gray: —

I intended to have had the books in your hands before this; but the remaining sheets have not yet arrived from Philadelphia — I think that Monday or Tuesday next, the Express will have charge of them for you — My Introductions have come, and I will send on some of them at the same time. The measures your Society are to take about increasing the number of Copies, must effect something, I am confident — As to Cost of increasing the edition to 500, I cannot say very definitely — The ~~plates~~ printing & coloring of the plates of the Coral volume (61) — may cost about 12 dollars a copy — cannot be less than $10. — The paper of the text (740 pages) (which is very heavy

- near $9 a ream) comes up to $2 a copy.
The printing, supposing the edition 500, about
(that is $4 for a copy of the text, &c $14–16, for the whole work, ea copy.
$2 more. — Other volumes will be smaller.
There will be, besides the Philology & Corals,

1. Birds Mammals
2 Botany
1 Fishes Reptiles
1 Molluscs
1 Crustacea
1 Geology
1 Geographical Zoology & Botany — perhaps 2.
1 — Natural philosophy (winds, currents,
Soundings, &c &c)

and in all 400 or 450 plates —

I do not give this as a correct list,
as there may be one or more volumes.

The Maps of the Expedition have just
arrived here, for our College Library, and
I presume you will find them at Harvard
They are sent by Bancroft. — If not there, they
are at the hands of the Collector of the Port of Boston.
You will find them every way honorable
to the Country. I wish you could see the
French chart of the Fejees, made by the
Expedition that was out at the same
time with us, and acknowledges us our
competitors in those seas. We had a better
chart from our Whalemen to start with, than
they have made. They were there but a few
days, and throughout the cruise went hurrying
along from point to point, aiming only to anticipate
us. We were nearly 4 months in the Fejee Group.
Not half the maps are yet finished. —

Our English Journals for Jany, somehow
failed to come, and we have written to
Hailey Putnam for a loan of copies —
I write in great haste — and will write again
with the books.

Very sincerely yours
Jas D Dana

Prof. Gray
Harvard

Prof. Asa Gray
Cambridge
Mass.

New Haven – March 23. 1845 –

Dear Gray –

As stated in my last to you, I left for Washington a week ago monday evening; and wednesday evening at 8 o'clock was at Brown's Hotel. I was not three minutes in the house, before I started for Judge Jaggar's room, determined to bring matters to a point at once. I soon made known my wish about remaining in Washington, stating my reasons. He then took up another topic, suggested by one of the reasons given, and went on to enforce the doctrine that Exped. discoveries were Exped. discoveries. And names given in Europe to any of the new discovered species were not to be regarded by us. I first argued the point; but he stated it as his order that it should so be. I gave him my downright refusal to go on with my duties under such an order. He said he should get a substitute then. I told him that I'd bring it before the Library Committee; and before a higher authority still, if they would not sustain me. – Whereupon after silent consideration for a moment or two, he began to explain away his order & finally left it for me to do as I thought best. – In reverting to the question of my living in Washington, he yielded that point also; and in less than 24 hours after my arrival I was out of the hated city again. I saw Wilkes and found him gracious enough. But the same subject, naming Exped. species, came up, with the same argument – and sustained against the British Assoc. & all Societies of all countries, as I told him. – He was impenetrable and as there was no occasion for a quarrel I talked on other topics. – I found your letter there still unopened, and, although the necessity of it was done away with, I concluded to hand it to Jaggar. – I assure you of my warmest gratitude for

your kindness — I now start again, clear of all incumbrances and need expect no further trouble or interference from that quarter. — Wilkes has the letter & the laws to shield himself in this trouble; and Jaggar has told me that he yielded the point about the book as soon as he had my assertion with regard to the it identity with the ms. approved. As a subject for an attack, you will have to give it up. There was an evil Spirit at the bottom of it: but with the defence which both Wilkes & Jaggar have, there is no occasion for a good substantial quarrel; and I think you had better pass it by. I shall give Jaggar yet some trouble for his last order to Washington, as I shall have it in my bills for travelling Expenses after a while. It was sheer spite & vexation that he had to yield in the case of the book, that led him to give the order. I hope we may get rid of him & Wilkes also. Such men as they urge are too absurdly ridiculous for men in charge of the Scientific publications of Government. —

I intended to have called on Mrs Hale on my return, but passed through Philadelphia without stopping, in order to reach home Saturday night. I however dropped her a letter, asking her whether she would object to the reclamations for Mr Hale — stating the plan, and requesting her, if not to send to you his Journals and give what information she had on the subject. — You will probably hear in a day or two. — Do not speak in your review of the claims of the rest of the Corps, for it would be horrible to make us responsible for Wilkes' Science. — His citations from us are few & often, indeed generally, full of errors, — But it is different with Hale. — The first important citation, is that respecting the negroes of Rio Janeiro. — You will see that Wilkes states that the information was obtained from Mr. Hale. — Hale was much provoked, that Wilkes should have used the matter in his Narrative, as it thus robbed his report of the information, intended for it. — He was making capital out of other men's labors and at their expense, even allowing that credit was given as here: for Hale had a Department to carry out — and report upon & was not in the condition of the Naval Officers whose Journals, were perfectly placed at Wilkes's disposal. Now there is much, very much, concerning the Samoan, Feejee, and Kingsmill Islands, the last two groups especially, taken from Hale's Journal — — I requested my Hale not to mention my name in connection with this move; and you can intimate that the information was obtained from him direct — although that name be not mentioned. — I would not accuse Wilkes for dishonesty, but speak of his work as well known to be a compilation from the Journals of the officers; and then go through, claiming Hale's part for him — Do it in this cautious way, without bringing special charges on Wilkes & he will feel it more, than he would any thing more abusive. He is proof against abuse (as you would see if you knew him) for he believes that it carries with it its own refutation; but a calm statement of facts would trouble him amazingly. I have requested Mrs Hale also to write me, here notes on the subject and I may have a letter before the addressed you — in which case I will inform you of it. — But I must hasten to a close, as the hour for the mail steamer coming —

Very sincerely your friend
James D. Dana

Prof. Asa Gray

Paid

Prof. Asa Gray
Harvard — Cambridge
Mass

When you have finished with it. I should be glad to have the Mem. returned—

New Haven June 17. 1875—

Dear Gray—

I received your kind letter some time since, and found every thing satisfactory — one single doubtful point remains — you quote from page 159 — and my author from 255 — as follows, Pontopp. Norg. Natural. i, 255, nr. 10. t. 14, fig. 5. — Can you reconcile it? — While I am much indebted to you for your kindness I am going to beg another favor of you — In the first place, however, a word about the notice of the Vestiges, which Silliman put on my shoulders in a letter to you. It is true that I penned it, but I had before me a long notice by the Professor — which was the basis of it — for I had never seen the book itself — Prof. S. criticised some few geological facts, but not the fundamental error of the work — about which he only expressed a certain amount of doubt in the notice. I have myself no faith in the author's views (derived from the German school) and none in Müller's peculiar theories, with regard to which Silliman's letter to you seemed to express for himself — favorable opinions. Yet I think it is well that such points are brought forward & discussed, as the Truth will be the sooner established. All the possible false theories must be proposed and supported with every form of argument that mind can bring to bear upon them, before the rubbish will be wholly cleared away and the truth be generally perceived and its full influence felt. Such works I would oppose neither with close argument that ridicule may console but will have little effect in convincing the mind that can be led astray by such speculations. — I regret that the few words said were not of a more conservative character but still think that no permanent injury will result from the wide distribution of the work. — As to the favor — it is simply that you will read and give me your suggestions & corrections with regard to the enclosed. You may think that I am getting a little Müllerish — but I am convinced that the mind of appears to afford the basis for a complete overthrow of Müller's theories — I was led into the speculations by the singular forms of life & modes of development in Lophytes. I trust you will have patience to go through with it — especially as Botanical facts are concerned — and apparently illustrated. I wish especially to know if I am right in such statements respecting the facts in your Science

or am made in the course of the article, and whether the principles are borne out by your knowledge of vegetable physiology. — No new explanations will be necessary to enable you to understand it — I wish I could be by your side & talk it over, or give you the preceding chapters of my Report on Zoophytes — but as that may not be — at least at present — I go on with my explanations —

1— The structure of a polype — Figure 1 — is a coral animal or Actinia, — a crowded series of tentacles, in twelve (three irregular ranges (like the petals of an artichoke) surround a disk with the mouth at the centre — the mouth opens into a stomach, and through the bottom of the stomach into the general visceral cavity which occupies, the whole interior of the animal exterior to & below the stomach.



Figure 2 is a transverse section through the stomach — the stomach is connected with the sides of the polype by fleshy lamellæ, which divide that visceral cavity into small compartments — Other intermediate lamellæ are much narrower — The compartments correspond each to a tentacle & communicate with the tubular interior of the tentacles. Below the stomach the fleshy lamellæ have the inner margins free — some are bordered by a white, convoluted filament which appears to be spermatic and others by clusters of ovules. The animal expands itself by taking in water at the mouth, through the stomach into the visceral cavity, & injecting it into the tentacles & throughout its structure. So much for the Coral animal — There are others much simpler without fleshy lamellæ within — There are the Sertularians & Hydras — but I will not stop to describe them —

2. Mode of budding — Some of these zoophytes grow, as in the annexed figure — representing a type of a branch of a madrepore — Each prominence (or calicle) corresponds to a distinct polype — having a separate mouth & stomach, though united to the others by the side tissues — In this figure there is a terminal polype — parent-polype — which keeps elongating indefinitely and giving out young buds from its sides. —



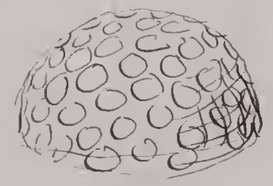
In other species, there instead of one parent polype there is a parent-cluster which continues budding, and thus produces a long thriving stem. The polypes of the cluster successively after



became a bud, a certain time became, after a certain age, buds, and these gradually pass out of the cluster to form the lateral surface of the branch



In another mode, each apical polype gives out only one bud, and this another and so on. — There is a distinct spiral arrangement of these buds, — sometimes from right to left & the reverse — and in their species there are 6 polypes to a single turn of the spire.



There are other masses which proceed from the summit of budding buds produced equally in every direction and not limited by the age of the polype — this produces hemispheres — Each new bud As the surface enlarges, either the intervals between polypes must increase — or else the disks of the polypes must enlarge — this increase has its limit, for in those species in which the intervals begin to increase, a new bud opens when this increase begins, in order to fill up the space & prevent the intervals from exceeding much their normal breadth — When the polype-disks enlarge — a new mouth opens in the enlarged disk, and the polype spontaneously subdivides, by growth as here shown.



In an Antipora the polype sends out a little creeping shoot from its base as annexed, which after growing to a certain length develops a new polype — and this in turn gives origin to other shoots and budding polypes —



— In the Gorgonia verrucosa — (the growth of which is shown in the annexed figure) budding goes on continuously at the apex of a branch — and as the termination of the branch elongates, branchlets form on either side — usually at nearly regular distances from the summit. These branches (as the dotted shows) & branchlets have on their sides a very large number of polypes in confused ranges. A branchlet begins by one of these polypes becoming a budding polype, which it cannot do until removed a certain



distance off beyond the influence of the apical budding polype. — This gives a system & regularity to the rule of ramification — and the same principle is illustrated in all branching Zoophytes. I wish I could explain to you the whole subject from specimens — but I must leave much for you to guess out — as my paper is already nearly exhausted —

The little Alga figured — Can you give me the name of the genus — or even a species if described — The dissections are made in the Recifes —

If you have (Zoological & Medical) a friend at hand, who is interested in such

Prof. Asa Gray M. D.
Cambridge
Mass.

physiological discussions, and is authority on the subject, I should be glad to have your united opinions — But I wish you would not show the Mss to others — I am daily expecting to have my Mss. sent for in order to commence printing — and you will be very much oblig me, if you could give the whole an early perusal — Excuse the liberty of my request, and

always make as free use of your sincere friend James D Dana

New Haven — Feby. 6 - 1846 —
Saturday

Dear Gray —

I give the book, to the Express this morning — Some sheets, including the preface, a new title page, some of the last forms, & others that have been reprinted are yet behind. but — I send on a copy of the Chapters on Structure (one of my extra Introductions) which is complete for that part of the work. I will send others of these Introductions next week; and if you will return the one sent, I will have it done up like the rest in boards. Keep it however, if you prefer it to bind it for yourself more substantially. I regret that the Preface is behind as it would give you a few facts with regard to the number of species or so. I will send it on, should it come, with the Introductions — The work is a complete treatise on Zoophytes — and appears to the

consist to a very large extent of non-expedition matter. But in fact, with few exceptions, the whole is based on Expedition information — Errors in description of species & in the laying down of genera, were numerous in the books, many species were compounded under a single name & the same name had been differently used by different authors. I charted not describe my own species, which, were in the principal suborder nearly half as many as all known, without giving the characters, more definitely, of those known. I could not connect the errors in any more concise way than by describing anew. Patching a new species to an old system, which the facts could not sustain, seemed, not to be my duty — The observations made were as important, for correcting errors as for instituting species, and I have consequently undertaken to reconstruct the science, revise, correct, and systematize the whole. —

I allude particularly to this, as it has been said that some of the Congressmen will or may object to the book on the ground of the matter; not being (appearing to) of Expedition collection. — Too complete! —— I have made out something in Ocean migrations for the Journal of Science, which will appear in the May number. ——

We can refer you to no memoir of Nicollet —

As ever truly yours

James D Dana

Prof. A. Gray —

I was about to pay for my package on — but Silliman has dissuaded me from it, saying that the N. American Review will bear the Expense — Tell me if it is so, as I do not wish you to bear the Expense of it. J.D.D.

Prof. Asa Gray
Cambridge
Mass

Dear Agassiz

Your communication from the Acad. of Arts & Sci. shall be presented to the Connecticut Acad. of Sc., — a society that has good names in it, & which meets once a month to talk Science — Do you intend that other Societies should report to you on the subject, or direct the Library committee, and had your report (or the same in form) better be signed by a committee here, or another form, alluding to your report? — Prof. S. is away now, but we will have his name on it. — Your Document is a very excellent one. — I would only suggest that where you say 50 or 75 copies have been published, you drop the 50 & — 75 (or say 75 or not) copies of my book on Corals, Lea & Blanchard consented to publish, and speaking of what has been done that number should be used; though it is doubtful if they would publish as many of the volume on a less popular subject — They intimated as much with regard to the Crustacea

Conrad

I was the remarks about Dr Hartley, because you may think it [illegible] & pertinent in connection with Hall's work (and mine, if it may so be) — and I wish you to appreciate pretty fully the character. He [illegible] in many [illegible] but although finding fault with them — and when any injustice [illegible] was being done, after the Cambrian rocks had been [illegible] when he had no prejudices, the decency [illegible] Dear Agassiz. No scientific [illegible] are they denied, after the [illegible] point which Courtney [illegible] complaining. — Had Wilkes been [illegible] his own ignorance on [illegible] Scientific subjects, he might [illegible] have made many errors in his narrative, and some [illegible] disputed conclusions. — But [illegible] variety has its returns. — It [illegible] the common opinion among [illegible] the naval officers, we [illegible] that if 6 worthy notes had been previously to some of the [illegible] is unfortunate, they might [illegible] have prepared themselves on [illegible] any of the departments of science, & they would have made no citation [illegible] Sciences — They would acknowledge [illegible] that it took at least [illegible] 6 years to qualify a navigator! [illegible] I can make the end of my [illegible] [illegible] you when [illegible]

I am much provoked that I must add a word of doubt as to whether the Coral volume can be properly reviewed in the next number of the N. American.— Because, the bills with regard to it cannot be signed till Dappan comes from Ohio (which they say will be in 10 days). Wilkes thinks he sees in the book a large amount of non-Expedition matter, and writes that his power does not extend so far as to allow of his signing them bills. When this news first reached me, I was vexed I had pulings as hard as a brickbat.— But I suppose Wilkes is right.— Dappan saw the manuscript, read it for three days in his hands, and finally gave it his approval, remarking at the same time on the description of species not collected in the Expedition. So that I am safe, if there was any disposition to make trouble.— After the correspondence on the subject I should not wish to give the book for a review before it has been presented to Congress.— Perhaps you had better prepare it, and if I hear about it in 10 days or so I will let you know.— Hale's book is not under this incumbrance — though actually as much liable to the objection as mine, and the review of that can be published whether mine joins it or not.— My material, the result of Exped. observations was sufficient— for a reconstruction of the Science, and I have consequently made a complete overhauling of the whole.— In no other way could I have brought out the results.— The Title page has not yet come; but I am still expecting it.—

The plates are yet in the works, and not— even half a dozen are finished— and none of those are here. It will probably be 8 or 10 months before they are all engraved.— They will be hurried, as soon as we have our next appropriation. They ought, all to have been finished before this.

I will write you again the first news I get from Washington. The next number of the Journal Contains two citations from the Coral book— one on the analyses of Coral, and another on the Cyathophyllidæ. They were printed before I had heard of the delay at Washington: and if they object, it cannot be helped.— There is no review of it.—

The bundle of extra copies of the Introduction shall be sent on as soon as I receive the remaining sheets from Phila.— The communication

to the Library Committee shall be returned to you by tomorrow's mail. — —

Yours truly
James D. Dana

Prof. Asa Gray

Prof. Asa Gray
Harvard — Cambridge
Mass —

although cumbering with his officers, and executive, exhibition through the entire a wonderful degree of energy and was late even to rashness in many of his explorations. I know no one else that knows as well, generally as I, that I much doubt if you will ever commander that could have been selected, we should have had better, or kind together more harmonious and I am confident that the library ever but contain a more daring explorer, or diving officer. —

P. S. If you examine Wilkes's charts, you will find them well done. They are the surveys of his officers (as well as himself) and among them were some excellent surveyors — The Fuegian chart is very far superior to the French one by D'Urville, made after their late voyage, a rival your expedition. Indeed, we had a better chart from our traders there, to start with, than that by D'Urville. His was the work of a few days, & ours of 3½ months. — I mention these particulars, because, what can may be said of him & the Narrative, this hydrographical department has been well carried out — Wilkes

New Haven
Wednesday Morning – July 15. 1846

Dear Gray –

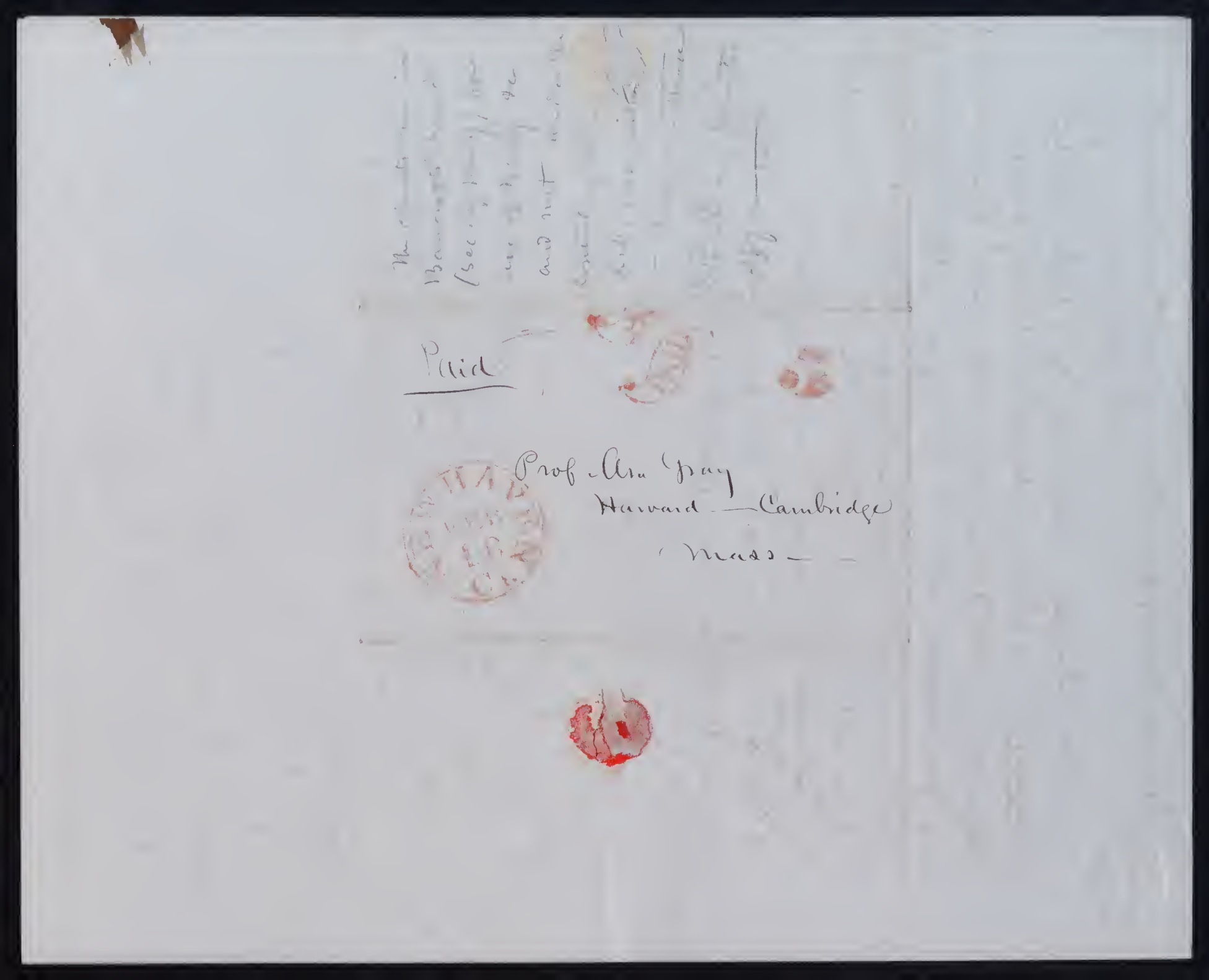
I am very provoked that the
revision of the Coal book must be deferred – I see the
importance of bringing it out at once – But Lapham
has been sick in Ohio, and although there is every probability
that the matter will be settled by April, it had better be
laid by for the present. Yet I am expecting soon favorable
news and when it comes I will write you. I like
you for the bill of the law which provides for the
publication of Exploring Notes – Lapham fortunately
saw & approved of my MS, and thus has relieved me
of all responsibility – The Hydrographical maps, or
some of them, I will send by the Express this morning
or tomorrow; but I must ask you to return them
soon, within a few days if possible, as I am
now writing my Geology of Coal islands, and require
them for reference. – The unfortunate bundle
containing the remaining sheets of my volume is
still on its way here, and the last news I heard
of it, was its being in Baltimore with Prof. Silliman

A month's delay of it has been occasioned by my requesting the Printer to give it to Prof Hallowell to bring on. Prof. H. left on day before the day he had specified, and about a dozen letters have since been written about the trouble. — I hope to see it by wednesday or thursday — I congratulate you in the close of your Scientific Labors. — and remain

Very Sincerely Yours
James D Dana —

You will find in the next number of the Journal a short notice of the meetings — I thought it best to allow that the hypothesis established would withdraw the argument against revelation, which is my opinion — But the whole thing I show to be groundless — utterly so — The Sequel is a splendid book for a reviewer who might wish to give the author a trimming — His attempt to show that Scientific men are incapable of judging his views is specious; but at the same time cowardly, and he deserves a lashing for it — However I view the subject as one for calm scientific discussion — which I think is the only mode of treating it proper for a Scientific Journal. — ~~J. D. D.~~ My last argument presents I have cut down so much, that I hardly think it will be appreciated — — — J. D. D. —

Our Librarian says that the charts are here on deposit — and he does not feel at liberty to allow them to go out. I regret it — but I am quite confident if you should call at the Office of the Collector of Port of Boston you would find a lot of them. Write not your President — Mr Everett — write on to Webster, and I am confident you would have a set for Cambridge — The sooner the better — Yours truly J. D. D. —


Paid

Prof. Asa Gray
Harvard — Cambridge
Mass —

New Haven – Feby 20. 1846

Friday –

Dear Gray: – I have at last got my title page & table of Contents, with the closing forms of the volume – and two of Wales – which I shall give tomorrow to the Express – I have also Signatures 29, 30, 40, 52, 72, 107 & 164 – for my volume which have been reprinted (more a month ago or more); but as they will be of no particular use to you I retain them – The two first you have report in the separate copy of the Introduction – I have packed up half a dozen copies of the Introduction (or Structure) for you and send the whole with other copies for Bostonians to be sorted – to save you the trouble of distributing – I have put up a copy for Decaisne of Paris & for Forbes of London – but for no other Botanists to go with the Journal of Science. – Do you handle them

is a copy of Dr Hann, which you will oblige me by giving him. —

I hear nothing yet from Washington — It is most provoking. — Had you not better have a review of Hall's in the Cepil number of the North American; it will give you an opportunity to express the opinions you suggested. — As you say, there is no time to be lost.

On mind about the plan of my book. — I have considered corals as animals, and whatever characters belonged to the living Zoophyte have been mentioned first in the descriptions — afterwards, if any other characters of importance were presented by the coral, (that is, characters not determinable except when it was stript of the fleshy portion,) they have been given — As with an animal, the animal as a whole is first described, and then any peculiarities of the skeleton are mentioned. ~~For~~ coral is in general an internal secretion; you might as well say that a man lives in his skeleton, as that the coral contains polyps. —

I have not inscribed a copy to the Academy, as I have supposed that they would undoubtedly have the complete work. Yet tell me — had I not better send one? —

Yours Sincerely yours
James D Dana

Prof. Asa Gray
Harvard —

P. S. — The bundle for you at Gor[illegible] is a heavy one — 7 copies — I have desired him to send it to you; but perhaps you had better see to it. — yours D. D. D

Prof. Asa Gray
Harvard University
Cambridge
Mass

March 2 — 1846 —

Dear Gray

I send you with this a copy of one of my plates — tho' not yet lettered or colored — It is the one referred to in pages 41, 42, of the volume on Structure and ought to accompany this volume. —

I presume you have on this Sea a copy of the Journal. It was intended to have it out tonight at least 8 days before the close of the month, that it may go by the steamer of the first; and if you should have any thing for the Intelligence Department, which I hope you may, it should be here by the first of next month, or very soon after. Any Articles should be in hand 6 or 8 weeks before the time of issue. — You see that we have the head Zoological researches — It possible it will be continued; and the same in Botany would be very desirable. —

I have heard nothing from Washington — I shall be glad to see your review of the Vestiges — You will observe that I have given it but a very brief notice

I have been requested by Prof. Norton of Delaware College to recommend Mr Harper (of Albany, now with Liebig) for the Rumford Professorship in their institution — In preference to writing Mr Everett, as I was requested, I state to you what I know — which is — that I know but little

about his qualifications. I have liked him much as a man, and believe that he might qualify himself. He will make a pleasing popular lecturer, and is in a school to perfect himself in Chemistry. — — Is this, as you think best.

I shall not have any use for my book, and you can keep the copy till your review is written —

In haste, Truly yours,
Edward S. Dana

Prof. A. Gray.

Yale

Having been requested to make a note to Mr Everett recommending Mr Horsford as above mentioned by Mr Dana I prefer to request you to state to Gov E. that my knowledge & opinion of Mr H. is quite the same as that of Mr Dana as expressed above Yours Truly

B. Silliman Jr.

Prof. Asa Gray
Harvard College
Mass.

Mikes demurs against
the publication of Extraneous
matter.

A pretty fellow he
who has filled up his volume
~~with all sorts of stuff~~
not only with what he
has stolen from his
corps — but with all sorts of
stuff. borrowed —

He is a good fellow in his
way, but cannot be endured
without, like Bottom — playing
the part of Pyramus &
Thisbe — & the lion — & all.

2

Prof. Asa Gray

Botanic Garden -

Cambridge -

Private

New Haven — March 7. 1846 —

Dear Gray —

I have had an elevated letter from Wilkes in reply to one from me in which I stated what was contained in the last number of this Journal. I told him that all was then inserted before the new discussion, & how it was too late to rectify it. The article in Contarea was reprinted, in order to keep the whole Exploring Expedition, with special reference to Wilkes's feelings & opinions, as I thought he might not think it expedient at the present crisis to have a publication of Expedition discoveries appear in the Congress Library, when the Journal is sent. I told him this; yet he is surprised at the course taken relative to publishing notices of the Contarea without consulting him (Judge Tappan) and without giving the Expedition credit for them." — So much for my kindness. — He says that the "new difficulties may have an influence on the decision of the first" — as if these two cases had any connection, Such ideas of Justice do not augur well; but the matter is not in his hands. I have told Tappan that I rely confidently on his full approbation of my labors. I have yet had no reason to doubt him, and I do believe he will set matters right. I wrote Wilkes, in reply, telling him that I should send his letter

with my reply to Jappan + for he has informed me that he should show mine to Jappan) — I also sent him a Copy of Jappan's permit to Sedgwick to publish a short description of the new species (Opossum being [illegible]), in some Scientific Journal. — I also stated (on his further information — by the way, there is another point: I wrote him that I felt satisfied that the presentation & approval of my manuscript, or even an order to print, [illegible] all responsibility. He disagrees with me in that point, he says, and adds that Jappan must assert that the printed matter corresponds with the manuscript He chuckles in vain, for in all the objectionable points, it does throughout correspond. It differs only in some verbal alterations and some shocking additions which have not changed it one iota in the points to which he has referred. — Well as above I commenced to say, — I also wrote him, for his further information, that I could show him the greater part of the identical manuscript, presented to Judge Jappan if he were desirous of it. — My letter was brief, and decided, without any of the personal feeling. I had -& [perhaps] there exhibit. — It is now for Jappan to decide, [illegible] will have no more words with Wilkes. — It is hardly possible that he may make some objection — If so my plan is to obtain letters, from those who know, approving of the book, and me as far as their consciences will allow, with introductions to prominent men at Washington, including the Library Committee, especially the Chairman — to go on there, see matters right, and then have a sub committee for publication appointed, for the Scientific departments. (Such committees exist in all publishing Societies, and why has there should we not have an ignorance) in Science to say what shall be & what shall not. Wilkes is well enough in his track, but poor enough out of it, though he has plenty of presumption & conceit. — I shall take no steps like I mean further. —

Another thing, in justice to Hale, I wish to mention, with the hope that it may be set right in the review of his work. Nearly all that is valuable respecting the customs, religion &c of the Feejees Kingsmiles, in Wilkes's book was taken from Hale's MSS. — The information in the regions of Rio Janeiro was all obtained by Hale for his own book & he got Agate to make for him the portraits inserted in Wilkes Work. I think you will find no credit given Hale by Wilkes for all this — I wish that Mrs Hale might be written on the subject, Hale's MSS. if possible obtained from her — that is the part giving an account of the Feejees &c — and a comparison made — that Credit may be given where it is due. — If this obtained from Mrs Hale, who can give full information about the whole matter, and knows of the injustice, I shall not be suspected of having had any hand in the matter. —

I have just time to get my letter in the Post Office before the departure of the mail with assurances of my high esteem from Yours Sincerely D. Dam

Prof. A. Gray.

Paid

Prof. Asa Gray
Harvard – Cambridge
Mass

New Haven — March 8. 1866 —

Dear Gray —

Jeffries Wyman has at last come and although he shows a hesitation with regard to the matter, he leans it still in doubt & advises me to come on. With a sum somewhat more I should have heard the further annoyance & expense. — I regret to trouble you with my troubles, but I believe you will give me a kind word if it will do good. And I write to desire of you a statement that the work is what it ought to be — Made in a Science in such an unsettled state as that of Zoophytes, patching in new species was impracticable without a thorough revision — My many of Lamarck's descriptions cover several of my species, and the latter could not have been described without redescribing those of Lamarck. Genera were also in a most uncertain state. & so on — You know the merits of the case. — He have authorized printing "Expedition discoveries, — So that You

must make it clear that the facts
by which Lamunch's descriptions & views were
connected, were not Egs. descriptions, as well as the text
and that the pen sketches, from cabinets at home, were added to complete the system
of species. — This statement will be of assistance
in carrying matters through, if Tappan resists,
although it does not suit the main point in to
the presentation & approval of the MS. — The only part
of the MS. not written when Tappan saw it, was
the introduction & Structure, and that is,
with the exception of a few sentences, original
matter: so that his objection must rest against
the other portions; and of this I know the MS.
to prove satisfactorily no essential alteration.
You know how much trouble a man with his
temper & will can give. I wish you would
send this statement to me at Washington by the earliest
mail, as I hope to be there on Wednesday or Thursday. If
the matter is settled at once by communication it will
not be used; otherwise it will come in play. Would
it be too much of a request also, to ask of you
to obtain — letter for me to some Congressman
who may have might write the Library Committee, from
Everett; — a letter that will lead the person to
take the matter in hand, in case it becomes necessary,
and not a mere formal introduction. I am not

personally acquainted with Everett; but a
representation from you will probably be sufficient
you are aware of the necessity of strong influence to
turn the attention of men at Congress to a subject
of this character. — Is it not strange, that
with the utmost, straight-forward honesty in
the while, I should have been subjected to
such treatment in the end! — If any trouble
comes of it, I shall endeavor strongly to carry
the point mentioned in my Saturday letter —
that is, the appointment of a committee of
publication, wholly independent of Wilkes —
I shall write to Everett for a similar statement
from him — Would it not be best to have
other names beside your own, to the one you
make out? — Will you send my book
and I would soon after the reception of this,
that he may see it, if he has not already,
and others in Boston. — Believe me
Most sincerely yours
James D. Dana

Prof. A. Gray

Paid —

Prof. A. Gray
Harvard
Cambridge
Mass.

New Haven March 11. 1846.

Dear Gray:-

You will be surprised to see that I am still here. But after writing you, I finally concluded that it would be better, instead of starting at once, as Tappan's "perhaps, the matter might be settled if I would come on" to write again stating that the Ms. was identical essentially with the last, and offer to go on and bring it if ordered. This appeared the more dignified course. I want to hear again. I shall take care that your letter to Washington, which I suppose has been sent, is taken from the office by a friend & kept till I see them.- I will write again when I start.- If I go, I do not mean it shall be for nothing, and I almost hope there may be occasion for it in order that a better state of things may be brought about. It is a subject in which the Country is concerned, and one which would not be beneath Webster to handle. Should you

See Gould will you let him know that
I am still waiting. — I am greatly indebted
to you for your aid & encouragement
most sincerely yours
James D Dana —

Prof A. Gray —

Paid

Prof. Asa Gray
Harvard – Cambridge
Mass

New Haven March. 16. 1846

Dear Gray —

I have just had another letter from Dappan in which he states that he has ordered the binding of the book, omitting the preface. — He grieves a little about these matters, and ends by telling me that for the rest of the time till the works are out — I must live in Washington! — This will not be, if I have to give up my appointment to prevent it. — I shall go on tonight, and hope to have that point settled; ~~when~~ It is perfectly absurd that I should be able to prepare my reports in a city where there are no books! — But such is Dappan. — A talk with him, and some decision, will probably set it all right. — Many, many thanks to you for your favors & sympathy — As ever my sincerely yours

James D. Dana

Paid

PAID

Prof. Asa Gray

Harvard — Cambridge

Mass.

New Haven March 30.

Dear Gray—

I received last Saturday the enclosure from Mrs Hale— which closes up that matter — I think her caution unnecessary, yet her wish must of course be regarded. Perhaps it may be better to take up the whole matter after the Reports are all printed — Wilkes, Semmes & Porter will find his level — After my last was gone I was afraid one word might be misunderstood — I did not imagine that there could be any real abuse of Wilkes, but only remarked that some & he among the number would consider abuse. The facts could not be stated without in themselves making a severe attack upon him. — I trust that difficulties with the powers of egression are done with for the present. — I have barely time to get this in the mail before it closes, and must therefore say my last word — and that, of continued friendship

from

Yours very sincerely
James D. Dana

Prof. Asa Gray.

Paid

PAID

Prof. Asa Gray
Harvard – Cambridge
Mass

New Haven — April 27. 1841 —

Dear Gray —

I have been hoping to hear from Washington of a resolution to increase the edition of the Exp. Reports — but nothing has yet come. I shall write soon to some Congressman and ascertain the probable course of things. — I am much indebted to you for your Criticisms — I only wish I were nearer you, that I might enjoy more of them. I was very much pleased with your review, and thought it an admirable one — I have never been afraid of the book, although I have not doubted that it would make some infidels. The supposed grand discoveries in E. Indian History, lengthening the age of this world since it was occupied by man, and those in Egypt, when first brought forward terrified many of the believers in the Bible. But they were the means of setting on foot a

To have advised that his business be omitted; they are peculiarly dreary, and your many as well, almost, make one's pains of Utes — as much as I have written in great haste, for which I ask your indulgence.

Dincan Greenleaf & Utes
assuming you of the
Yours
Jas D Dana

Dr Asa Gray

train of investigation which results in a vast increase of knowledge, and above all, in confirming the Bible history in every point. — In the same manner I believe that the Publication of the Vortices will be the means of much good to Religion & to Science in general. — I remarked that should this theory prove true, it would not affect the truth of Religion. — My reasoning is this. — That the condition & character of man requires for his improvement and Sanctification the principles we, as Christians, believe. and this fact will not be modified by any view of our Creation. — We are sinful beings, and no other plan could restore us. — The system should be one which will make us feel our dependence on God, perfect resignation to his will, and filial love & fear, while pride & selfishness are subdued. — Such is the end, as we know, of the Christian religion. It makes us children of a Heavenly Father, to whom we may appeal under all circumstances. — ~~without this spirit of dependence~~. The coming of Christ, as we believe, is an essential part of the system, and not only to satisfy Divine justice & give us above an example of perfect Obedience, but also to give us an Object of worship within our comprehension (or at least seemingly so) and this contract that tendency in man to form images & give shape to the invisible spirit of his imagination. — Now we know God's benevolence & love from every object about us: — and this admitted, we know that such a system, his benevolence would have established. This system is based on reason, though the finite reason could never have devised the plan. —

This argument from the internal character of the Bible-system, affords the best possible evidence of the truth of Religion, and would stand were we wholly ignorant of any plan by which we came into existence. And on this ground I have inferred that the theory of the Vortices, if proved, would not affect the truths of Christianity — Am Trig.

Hale is in Europe, but nobody knows where, not even his Mother. — It seems therefore that the reclamation must be given up, unless you merely refer to such parts of Wilkes's work as Wilkes himself acknowledges. — The Negro Heads is at Valparaiso. — 1st Volume — The account of the Kingsmill is Landers — Vol v, p. 229. —

The Journal of Science is just out — Many list of Lond. researches, & Later no price in, as yet, as our Journals have not got in the way of publishing

of being regular — I intend to keep them up, and make them more perfect before the year is through — Botany ought to be added. — I wish also to give as concise a statement, in each number, of the doings of our own Societies in Science — Publishing the full Proceedings as was done formerly brought a great deal of rubbish into the Journal — This is now superseded by the Proceedings

Prof. Asa Gray
Harvard
Cambridge
Mass

being published by the Societies themselves — But a brief statement of their doings, kept up systematically, should be regularly inserted — and hereafter will be attempted. — We are always crowded for room: — And sometimes have to sacrifice one department for another. — — What is the character of Dewey's articles on Comics — We have another from him, and

New Haven – May 23. 1856

Dear Gray

All I can learn from Washington amounts only to a probability that the edition will be increased to 2500 — possibly to 3000 —. The 2500 will be scarcely better than 100 — for it will prevent any publisher from undertaking it. and consequently as government will probably keep the 2500 for distribution there will be none in market, instead of 4 or 5000 as now. — I presume that if 2500 is the number decided upon, they will take Lea & Blanchard's copies, and let Hale's & my works go without reprinting — not thinking it worth while to set up the types anew for 50. more copies — The country & the world will therefore be a little better off than under the present law — However they may decide on the larger number — — I

forgot to state in my last, in reply to your offer, that I should take pleasure in seeing what you

write on the delicate point. — I should be
glad to visit Boston & Cambridge, but at present
have no duties to call me your way. I am driving
away at my Geology — have finished the
account of Coral islands — (about 120 pages of
another book) — and am now upon the Sandwich
Islands. —. What will you have for the
Journal the coming number? — Your initials,
were ~~[illegible]~~ added to all your notices of Botanical
works (except one I believe, in which the line happened to be filled
without it.) Hereafter if you wish it omitted will you
not mention it. — The number was overflowing with
matter & there was not all the room for bibliography
that was desired. Redfield is in the midst of an endless
storm — we have asked for a temporary suspension of it,
and the continuation may be depend on number — So
much entomology cramps us much. —

Sincerely your friend
James D. Dana —

Prof. Asa Gray

Harvard — Cambridge

Mass

New Haven – June 15 – 1876 –
Thursday A. M.

Dear Gray:—

The proof came yesterday afternoon and this morning is emailed. – Your review is an extremely happy one – beautifully written, very skilfully managed, correct, and – just the things You have made it much more attractive than I had supposed possible. Indeed you have a very happy style of review-writing I have no suggestions to make except the word or two on the proof. –

I understand that Tappan (who had been authorized by the Lib. Com. a year or two ago to have a duplicate of specimens from our duplicates) has been ordered by the present Lib. Comm. to return all that he has taken (Shells &c), and the law authorizing it has been rescinded. – The specimens belong to Govt., and the Lib. Com. never had any right to dispose of them; they might as well give away the duplicate works of the Congress Library. – It is gratifying to see that they have returned to their senses. – My gentle hint,

(request to a kick down stairs) led Sappan to think that his services might not be needed & so he resigned ——

I am glad to see that Peters speaks so well of Hale — In the 20 copies (extra) I should wish to have Hale's included. —

Very sincerely & gratefully yours
Edward Morse ——

I have read your Prefatory for the Journal with care, & I hope you may find no errors, though I doubt. — As the time of your MS for the Misc. is here by the 5th. (1st. is Sunday) of the month before the issue, proofs shall be sent. Anything for Articles (your review of Lindley) should better be here as soon as may be after the 1st of the month in which preceding number is issued — You will find that I have changed the plan of noticing Scientific Researches; and I wish to give in each number, regularly, the proceedings of different American Societies, so far as to notice the titles of articles, their authors, and time read — you will comprehend it from the specimen — Have we better Add to the list of foreign Journals particularly noticed, Hooker's Mag. — There is room only for the most concise statement of titles — In the following volume of the Journal, we shall take out all the headings, which will increase the room in the Journal one twelfth. — Yours F. W. P.

Paid —

Prof. Asa Gray
Harvard — Cambridge
Mass —

New Haven – Feby 17. 1848 –

Dear Gray: –

I have been absent (for nearly a fortnight at Washington and was absent when your note to me arrived. The Preface of your New Botany was also received during my absence, and a notice of it given to the printers. – Silliman mentions that your friend Carey will not review the work for the Journal. My abilities are small in the Botanical line: but whatever I can do, I would do gladly & perhaps if you would give me an abstract of such a notice as you wish desire, mentioning the points of interest, I should make out. –

I was sorry to hear of your afflictions. But what a check to all grief to know that this life has ended in a better – – or endless happiness. –

I am always glad of your criticisms, as I seek only truth, and I love the man attached to me who will help me to avoid error. But in this case I think you do not fully understand me. I do not mean to imply that there is an identity of forces in kind, in the animate & inanimate kingdoms. This is far from my belief. – I

merely states that a common law as regards the
force operates in both Kingdoms. — This is the Law of
interval or size — that is, that successive productions
are separated by intervals, usually regular (circumstances
the same); these intervals are intervals of comparative
rest and gradual growth, and are often intervals in size
as well as time. A length of interval may therefore be
a fixed quantity (cet. par.). — For Ex.ge — a certain
size is necessary for the production of a bud, and a
certain interval of growth — that is, of size — for
another bud. — In the little Alga, in my Zoophyte
chapter, sporules form only at a fixed interval or
distance from the Extremity. In a Zoophyte, branches
form at a fixed distance from the apex, & at successive
intervals; which intervals, cet. par., are fixed in amount.
It is the same in principle if the buds form annually at apex
There is something which determines these limits for
distances; and in the Case of the Alga & others it is
good philosophy to say that the process of growth
at the apex, will not allow (cet. par.) of sporules
forming within the specific distance. The chemical forces
required for growth at apex do not admit of that
different action of forces producing sporules within the
Specific distance. — The fact that size is an
fundamental element — as much as in a galvanic
battery — no doubt for analogous reasons — is well
shown in a brief article from van Beneden in the Journal
just coming out. — The Campanularia, Ascidia &
other species that bud & form compound groups, grow
to some considerable size by budding before ova
are produced. The young animal produces a succession
of buds or polyps, and after the dendroid group
has reached a certain size, then it produces
gemmules which separate & give out a free young animal,
of peculiar shape (Dif. from the polyps). This young animal produces
ova. — The ova again must go through the same
process. You know the analogy to vegetation
in which a series of buds usually form & the plant thus
attains considerable size before a flower (an
individual of any dif. form from the ordinary kind)
is produced, with the developing ovules. Steenstrup
has published a large work, wh. you have probably
seen, on Alternating Generations: — all the facts
of which amount to nothing more, essentially
than what is common in vegetable life, Size
in a Cat
& large a length of interval,
must therefore be an important element
of organic growth. — This is the main
point in my last
article. — Prof. Henry, on evening at Washington, states to
me that he considered the forces in animals nature, chemical
forces; but that there was a directing (mental) behind all
modifying or governing the results. He compared it to a
Steam engine, where forces within were directed in their
operation by the engineer. This is the view I have held,
or favored of late. In a chemical point of view, the
germ requires a condition of chemical forces, more unusual or
of a higher character than any other part of an organism; for
the product is in part those chemical compounds, which
are highest in the ascending scale — the highest of the proteine compounds;
and it is a just conclusion that these formations, or chemical
processes, attending growth, in different parts of a plant

should exert some mutual influence, and require some distance of interval. — But I will stop - as it is a difficult subject to write upon off hand. — I intend to put something together for the Journal, or perhaps for the next Assoc. at Philad. — I fear none of them will see above my views as they are, — they will [illegible] for I wish to give the subject a long thinking. — Any news from you on

Prof. Asa Gray
Cambridge
Massachusetts

the subject will be most acceptable. —

I am glad that you will soon find leisure to give us something for the Journal. — I shall probably send you in the course of a month or so, a continuation of my Crustacea. — —

Very sincerely your friend
James D. Dana

Your Proceedings came yesterday —

New Haven — July 13. 1848 —
Thursday A. M.

Dear Gray: —

In my last long letter to you I mentioned frankly the state of my feelings as regards Harvard and Yale, and announced that I had promised Silliman not to refuse a well-founded Professorship at this place, if offered me. I have had little expectation that any thing would be done, and this little has recently been on the rapid decrease: and I have daily looked for a word that would decide the matter Harvard-wise. — But yesterday there was an unexpected offer of so generous a character, that I could not decline it: and therefore, here I am, com-

am to be. I know that I need
make no apologies under the circumstances
for drawing off from my partial engagement
to good friends at Cambridge and Boston:
nor are renewed assurances needed to
satisfy them of my warm attachment and
gratitude. — Will you kindly explain to
them.

Your books were safely received, and
I hope to dispose of them all before long.
The plates are very handsome, and
the charge is most reasonable. — There is
a plenty of room for Engelmann's Botanical
Article in here soon. —
Article — Believe me

Ever

Sincerely yours
friend
Hamilton Gibbs.

Prof. Asa Gray. —



Dr Asa Gray

Cambridge

Massachusetts.

1848

New Haven – Aug. 29. ~~1848~~ –

Dear Gray: –

I intended sooner to have replied to your last. – But, in fact, I could not comply with your wishes, owing to my utter destitution! – For about three months, the Washington treasury had given us no funds. – We expected remittances have however at last arrived. – And now I must disappoint you again by my want of success in disposing as yet of the copies of your "Gen. Illust." I send you the money for two copies. – Prof. Norton was on the point of taking one, when a copy was presented him; and two others which I was sure of disposing of, are still on hand. – Dr. Tully is really poor. – His house-rent having been paid for him for a few years past, by some friends. –

I was very much pleased to

read the letters you sent me,
and gratified with the high admiration
expressed for your elegant work. —
Boott must be a noble-hearted man,

The plan with reference to my
Professorship ~~has never originated with Pres.~~
was the proposition
~~I thought~~ from the Literary Professors.
Aware that I might be called some
where else, the subject had been
talked over by Pres. Woolsey, had shown
much interest in retaining me at Yale,
offering himself to give $1000 towards
a Professorship. The Literary Professors
agreed to, knowing of some ulterior plans
for Yale that would render the raising
of a fund, for a single Professorship
at the present time
inadvisable, stepped forward individually
and offered to pay the salary himself
for three years — commencing 2 years
hence, until which time I should
be in the service of Government —
and longer if those ulterior plans
were not then consummated; or until
the fund for my Professorship was

* The 3d anniversary of my son. —

secured. Those ulterior plans
embrace the establishment of several
Professorships by raising a fund, among
3 years hence*
the Alumni of the College; and if the
whole is not carried out, both the President
and the Professor allude to assure me
that they will see that the fund for my
Chair is secured, & that this will be
the first one established. — With so
much personal interest on the part of the
President of the College, & such generosity
in another, I could not but be
highly gratified. — The mail here
coming compels me to close, at once
with assurances of high esteem &
kind regards to your lady — excellent,
I have no doubt as she is your choice,

I am as ever, your friend

James D. Dana —

New Haven –

May 19. 1853 –

My dear Gray: –

Your note of Mgt.
Came last evening – I will
speak about – for the degree.
He ought to have it; & could have
got it at any time if he had
been here at a commencement –
after 3 years after graduation –
usual
The charge is 3.00 – But I
presume nothing will be asked in
this case. Your article will go*
in & it makes our last for the
number, as the last bit of space
will be occupied – I mean of
Article – Space. ——— My

* Or Tetradia, n. sp. Verbenae, in your coll. also Erysi & Boulton. A.G.

D. is quite well again & the little one is as talkative as most babies —

In haste

Yours very truly

James D. Dana

Prof Gray.

New Haven Ap. 30. 1855

My dear Gray —

Your article on the Smithsonian Institution has come, and is just the thing for the Journal — well deserving the praise it has received. There is no one in whose judgment I have fuller confidence than yours. — I will see that two proofs are sent you — and probably will have a revise sent beside. — I am very glad you consent to take up the subject —

As ever, Yours sincerely

James D. Dana

Prof. A. Gray

I will see that two proofs are sent you to compare with the manuscript

P.S. We shall have a general Index for vols. xi – xx in the December no., which closes the xxth, and if you will suggest any additions to the Botanical or make analyses of Papers in your department, I will see that they go in. The General Index will be made up from the Volume Indexes. –

Yours

J. D. D.

I also propose to publish a general Table of errata for vols xi to xx

N. Haven
Saturday
June 23
1855

Dear Gray: —

I have rec'd your two last — with enclosures for the last. I have just sent for you a copy of the missing no. of the Journal: and any others one missing in your sets I will send them on — We shall have room for Brain & the glad creation it. — Can you send it next week? Our July no. will be sent off early next week. — as ever

Your friend
James D. Dana

over

all but the Coniferæ, it would be satisfactory.

Yours —
J. D. D.

P. S. Here is a point in Fossil Botany, I should like your advice about. It bears upon Classification. —

In the Coal Period – the prevailing species are Sigillariæ Lepidodendra, Calamiteæ and Coniferæ. All but the Coniferæ have been regarded as nearest to the Lycopodiaceæ + Equisetaceæ. But the later results, make the Lepidodendra near the Lycopodiaceæ and the others Gymnosperms. — Now there is no doubt that the whole number including the Coniferæ have a close relation, even viewed with reference to system – although having important differences. The great peculiarity of the Coal vegetation was its essentially flowerless character, the Conifers being next door to it. Now Geology finds it necessary to speak of the groups by some common name. — Mesozoic was called the Cretaceous, Tertiary, the Age of Acrogens, but if the Sigillariæ + Calamites, Lepidodendra are not Acrogens, the name fails. — Is it true that the Sigillariæ are Gymnosperms, (Hence while the Lepidodendra are not? as Ad. Br. has it in his later publications? I wish you would help me about this in an early letter, as I want much a general name for the Group. If the name were applicable to

New Haven – June 30. 1855.

Dear Gray:—

Mr Kline has called me, and says he has the translation nearly finished. — I should think it well to publish the "Reminiscence" if he will translate it. — I appreciate your word of Caution respecting the Hymenopterous;— Yet am far from disagreeing with your views. The wide distinction between the Endogens & Exogens in structure cannot be set aside, or lightly treated. — The fact geologically is simply this — that the

Perhaps Bronguiart, in the disposition he makes of the Hymenopterous rejects the consideration of much of force in his argument with reference to the Calamites &c Sigillaria – and if so this would falsify his conclusion completely. As far as I can judge, the structure of these Calamites is not Endogenous, however much they may otherwise resemble Conifers. However I have not paid special attention to the subject. —

Bronguiart has an article on the Calamites etc. in D'Orbigny's Dictionnaire Universel d'Histoire Naturelle. I should like to have your further views on this subject. –

Sincerely yours
James D. Dana

plant idea (after reasoning)
was first expressed in that division
of the Exogens called Gymnosperms,
and in the Acrogens; and that the
between the two, there were
forms nearly - though not
precisely-intermediate — Afterwards
the Exogen type was expressed
in a fuller, purer, and higher
character in the Angiosperms -
While the Acrogens stood their
ground, though dwindling in size,
and the intermediate tribes
genera dropped off altogether.

The Endogens appear to have
been a wholly new type, possibly
& probably not at all represented
in the Coal period, unless some of
the supposed flowering plants (Newberry

has fig. some from Ohio) are true
Endogens of small size. The Palms
are supposed to be Carboniferous,
have almost wholly disappeared
before investigation[x]. —

Bongniart makes the Sigillariae
and Calamites, relatives of the
Conifers rather than the Acrogens.
Wherein we used to think the
Calamites, most nearly related to the Equisetaceae
and the Sigillariae to the Lepidodendra:
and his conclusion is based on the
woody texture. — As the Sigillariae
are even more important in Coal
vegetation than the Lepidodendra,
this view spoilt the word Acrogen,
and I, I wish to retain it if it
will do. I suppose however there
is doubt enough over the subject
to allow of that term.

[x] Calamites remarkably seem to have been huge tree like [illegible] mostly [illegible]

New Haven, Aug. 7, 1855.

My dear Gray: —

I enclose you Prof. Darwin's Observations are particularly interesting. — I hear your article of the Smithsonian spoken of in the highest terms — will you be at Providence to see your friends there — I shall hope to meet you there —

Sincerely yours
James D. Dana

Dear Gray —

The mail has just brought the enclosed from Blodgett — I thought of replying that any misstatement respecting him we would correct, if such there actually be — But that we have acted on the principle & still intend to, of admitting nothing into the Journal on the disputed subject of the Smithsonian Institution except what is strictly editorial & therefore

should decline his paper — Perhaps as you are interested you had better frame a reply & send it to me by an early mail. — I thank you much for the word on the Sig. in pun Hooker — The fact that Morris in his edition just out his "Cat. of Brit. Fossils" plans Calamites, Asteroptyllites, Sigillaria under Phanerogamia Gymnosperms after Brongniart seemed to show that there was a strong leaning of Science that way Morris has it — thus —

Cryptogamia { Amphigena, Endogena, Alga
Acrogena, Filicina, Characea, Lycopod., Equisetacea

Phanerogamia { Monocotyledones — Cyperaceae, Typh., Pandaneae, Palmae —
Dicotyledones { Gymnosperma — Calamita, Asteroph., Sigill., Cycadea, Nöggerathiae, Coniferae
Angiosperma { Apetala —
Gamopetala —
Dialypetala —

Yours sincerely
James D. Dana

A. Gray

New Haven, February 8. 1856.

Dear Gray. —

I send a proof of Your Botanical notices. —

As to the point in my Review to which you allude, my conclusion was arrived at independently of the use made of it. There is no doubt that in the institution of the Plant Kingdom, there was throughout special reference to the higher Kingdom of Life — the Animal; — that the two are direct Correlates — that the one is made to play into the other in perpetual circle; and that this perfect balance of relations or interplay was the primal idea in the original expression of Life in nature. Now functionally & structurally this relation is exhibited in the fundamental quality on the part of the plant of taking in Carbonic acid as a means of life & giving out oxygen, while the animal on the other side takes in oxygen & gives out Carbonic acid. The whole structure of the plant in its most intimate nature is based on this relation to the Animal Kingdom; and the spirit of the Cattle required just this balance. Now,

it is an additional fact that this Counterpart in the System, the plant Kingdom, affords also the food of animals, and is indeed the means by which mineral matter is worked up for this purpose. Yet if we can separate two principles which run parallel I should plan the former as first and make this concomitant. The former would have been necessary any way, even if another mode of feeding animals had been adopted.

Taylor Lewis's work is one of the worst Infidel Publications that has appeared, altho he was not aware of it. For scepticism as to matter leads necessarily to mineral scepticism. His arrogant system pronounces Bacon's Novum Org. nonsense, Inductive philosophy a road to error, and men of Science chasers after bubbles. - His name was giving undue currency & weight to his work among religious men & clergymen especially and I thought it desirable to start him on his way down hill - to give him a push

, believing it right that he and his errors should find a common level in public estimation. — —

Sincerely Yours as ever

James D. Dana

New Haven, Feby 23. 1856

Dear Gray:—

I thank you for your letter of Explanation.—
I looked primarily at the Cycle made by the two Kingdoms, and viewed the oxygen given out by the plant as a very essential part of the pabulum of animals Still I think you are right in your view.— I understand that Prof. Lewis is preparing to fire back.— and although I dislike exceedingly controversy, I am willing for the sake of the truth that he should. A second presentation of the subject in reply to objections

is necessary to remove doubts that will hang about those not versed in Science. — Then as on a two points it might to have enlarged upon — and one, the fact that Newton did present theories that have been set aside — (theory of light) — Properly viewed it confirms all my principles, although there is seeming discrepancy. I presume Prof. Lewis will make the most of it. How phlogiston's & elephants are generated through Prof. L's method of studying Nature, and eternal laws out of the accepted method is a subject not as well understood as it ought to be. —

Ever yours, most
Sincerely
James D. Dana

New Haven, Apr. 21. 1856.

Dear Gray:—

I must trouble you by asking an hour of your time — as it is for the good of Science. —

In my parting remarks on "Science & the Bible" I wish much your revision and judgment; and especially on a page where I allude to Agassiz. If I am wrong in the statement, I must erase ~~that part~~ last clause of the sentence containing his name (p. 14); and again if you can in truth

express the same facts
in stronger terms, I wish you
would do it. I presume it
will not be necessary to
show the paper to him; and
you need not mention my
quotation from Prof. Peirce,
leaving it for the printed
page to make it known.

When you have read & made
your corrections please mail
to Prof. Park Andover, (under,
the Charges are such as you
wish to consult me about in which
case you can remail it to me.

I hope you can look it
over within the first 24 hours
after receiving it, so that there
may be no unnecessary delay.
The article will appear in the
July no. of the Bib. Sac. — I
shall be greatly indebted to you
for your emendations —

As ever sincerely yours
James D. Dana

Prof. Asa Gray

is on in species —
Man had his one birth-land,
and birth-place. — I believe
too, in his own first-generation, although here the argument
has to encounter the facts
under the head of incest. —
These incest-facts appear to
me to be the only hint from
Science against the complete
unity of the race. —

But I am taxing your
time. I wish you would
. Some time stop here with
me that we might have
a talk: and Mrs Dana
would give a warm welcome
to this fray. — My Jemmy
absence, is on a lecturing
trip, now [illegible] the year about 3 weeks, or 3 to 4. —

As ever yours
Jas D. Dana

Dec. 11. 1856

Dana's suggestion
with regard to sending from
the Journal is a good
one. I give the New
Haven to obtain the
Journal
Hastings for my own extras
to them. I have not always
done them. As
regards, it is a question for
the authors themselves to
decide. —

Dear Gray —

Your three
Extras were sent two days
ago, and I presume you now
have them. I was glad to
see Dana's letter. He is right
about your paper on Geog.
Distrib., for it is admirably
treated every way, fully,
fairly, and in a manner to

bring out the grand results at the basis of the whole. I do not know what Mr Darwin's opinions are on the subject of species. He is an exceedingly faithful man in his treatment of his subjects, faithful I mean to them as he is able to decipher them teachings of nature, as far as Oswald Heer seems to favor Forbes's notion of the junction of Madeira & the Continent — — a grand hypothesis for the Convenience of a few plants & invertebrates. — Darwin says he believes in centres of

x that is, as far as instances can be multiplied

radiation for species — I shd like some time to talk with you about it. It is a pretty thought, of Eden growth; but perhaps, there is a little too much of the straight jacket about it for nature. A centre of radiation for each separate species may to a great extent perhaps be proved. But my remark above refers to great groups of species having their Edens. — I have been thinking a good deal of late on the subject of races, and have strongly confirmed my conviction, that what can may be said of other species science strongly proves[x] that

New Haven, Dec. 31. 1856.

My dear Gray: —

A happy new
Year to you, and a thousand
blessings with it! —

I am up next- this noon, and
have time only for a word or two.
I like the views in your letter of
the 13th. — As to connecting Madeira
with either Continent, Geology seems
to me to be rendering the idea
less & less probable: and I think
we must reach higher laws of
Geographical distribution than that
of transfer for such cases. Yet I
am far from following the tendency
of the day to strike out transfer altogether

I have been disposed to attribute the sublemen of progeny from near relatives to a want of opposiiteness in the two systems. The strongest combinations in inorganic nature are made when the polarities are strongly opposites – if we may so speak.– There is in the progeny of a single couple a degree of sameness that does not fulfil the conditions, even when there is perfect health. There are polarities of one kind or another all through nature.

But – I have not time now for a discussion. – We shall be able to fill up our Journal without your paper

Our Salem subscription to the Journal has just fallen off from 6 to 3! – I wonder if there is any way of recruiting for me grow a little smaller every year as far as city agencies go.–

I will send you a photograph for yourself and print in London in the course of a month or two. – Ever

Sincerely your friend
James D. Dana

Dr Asa Gray.

New Haven, Feby 9. 1857.

Dear Gray:—

I send you the Stamps — $6.50 — — Your Mss. will be in type today or tomorrow. Have you seen Taylor Lewis's New Book? — It is despicably abusive — e.g. "Voltairian clamor of science" — Prof Dana's is in some part of his article "impious" — & pantheistic — more than the Vestiges or no matter — as pious as the pre Thinkers of Germany &c — He winks as if he were mad all the way through. — I shall give him another dash of cold water

and hope it may do him good.
"Bible & Science" is his title.

The photographs will be
ready soon. I hope you will come
to see us some time. —

Ever yours sincerely
James D. Dana

Prof. Gray 3

My dear Gray:—

The notice of your excellent little book was received a few days since, and yesterday, another of Holton's work. I like your "Lessons" very much. Your illustrations are very tastefully managed, and the work is — real knowledge made "real easy".—

I should be glad to obtain some photographs of the Damants of England — and would ask now for the Bartlett. — I am having an engraving made from the one good photograph, and when Completed, if well done, I will send

you a few and will then accept
of your kindness in the presently of
some from abroad by way of exchange.
The students of a graduating class
have a certain fee of adding from
time to time to the engravings of
Professors for their albums; and in
nearly all instances the engravings are
miserable, being cheaply done by
a poor artist of this place (Prudence -
the engraver of the take-a-daisical
or rather stupid picture of Dr J. Wyman
in the last number of the Scientific
American); — And I have wished
to anticipate any such maltreatment
of myself: for such pictures are the
worst sort of libels. —

Sincerely yours

James D. Dana

New Haven, Apr. 3. 1857.

New Haven, Mar. 11,
1857.

My Dear Gray: —

I drop you a line to acknowledge the receipt of your Ms. and its passage from my hands to the printers. — — I don't know what you will say about my waste of time over Taylor Lewis, when you shall see what I have been writing. The April no. of the Bib. Sac. will contain the first portion of my Article, and the July no. will take in the rest. I have wished to explain at some length what

Science teaches about nature's individuality, and the limits to man's speculations about nature, and this will make the better part of the article in the July no. In the first part I show up some of Th.'s ridiculous positions. I look upon Prof Peirce's method of philosophizing from nature upward as very audacious, and I have therefore the more wished to draw the line which bounds the region of pure speculation, & alias, absolute ignorance.

as ever sincerely yours

James D. Dana

New Haven, Nov. 13. 1857.

My dear Gray:

I always like your criticisms, for I am sure to find good in them, and I thank you for the views you have given me. I think however that you have missed the bearing of some of my statements and I will explain.

In the first place, my argument was intended as an argument and not as a demonstration. I state that the mind requires positive facts from the special side before it can be fully satisfied. The course of thought, if appreciated, will excite to greater caution in the study of facts and to more far-reaching vision in the comprehension of them.

The ~~method~~, which I have followed in reasoning, from inorganic species to the organic does not depend for its truthfulness or force upon any particular notions respecting the former. The great truth is this; — that there are mathematical contents at the basis of the inorganic kingdom, that is, fixed units under mathematical law, and hence the conclusion, in view of nature's unity, the necessary conclusion, as seems to me, that there are fixed units in the organic kingdom. This is further argued from the necessity of such units in any system intelligible to Man, and the ascertained fact of finite numbers in the foundation laws of nature, — that is, in all the laws of molecular forces, upon which both the inorganic and organic are based. I therefore conclude that to attribute

indefinite blendings ~~to~~ any part of the Kingdoms of life is "unreasonable".

The argument does not prove that there may not be among species a law of multiplication by definite combinations. This "spirit of my own raising", which I acknowledge the subject suggests, I meet by urging first that no facts have hitherto led even to the supposition of such a principle in the organic Kingdoms; and secondly, that the law of combination by definite proportions in the inorganic Kingdom is a general law in that Kingdom, the very spirit of the system; while in the organic, the system is not penetrated by it; any supposed instances of conformity to such an idea are not exhibitions of the spirit of the system; they are exceptions, such as we should naturally reckon among the results of "variation" under law, and not as direct expressions of a comprehensive law.

Definite blendings, if such were possible, would have a definite manifestation in an intelligible system of nature like that open to man, and the law should not be difficult of discovery.

It is certainly no objection to the deduction of a mathematical basis for organic nature from the analogies afforded by the inorganic that a misuse may be made of the analogies. Besides I do not regard ~~Besides~~ it a misuse to infer that since an inorganic species; as iron or oxygen, was created in a vast number of individuals over the earth, therefore organic species may have originated in many individuals. I am inclined to accept the inference, make the law general, and derive its limitations from an actual study of nature. I see no violence to any admitted a-priori principle or ascertained truth, in the view that the species of Infusoria & Cryptogams — those species of the Kingdoms of life nearest in grade to the inorganic — were actually created widely over the globe, as their geographical distribution suggests. And the distribution of the fossils of the early geological periods, as well as certain facts among recent species, indicate, that some Mollusca & articulata may have had a plurality of origin. But as we rise in the grade of species, geographical distribution demonstrates that in each case the original creations were confined to increasingly narrow limits. These limits narrow so rapidly, as we ascend the scale, that among Insects, even granting that the doubtful cases stand on the side of a multiple origin, we have still very few intimations of such an origin; — that is of a distinct origin in distant & disconnected geological regions, as, for instance, in the two temperate zones, north & south. As regards the superior Vertebrata, the intimations are ~~Much~~ much fewer, and more sparing still among the superior groups, so that it is almost an established law that a species of these groups has not been created in more than one zoological region, if it be not also demonstrated (which can hardly be asserted) that each is of one parentage. Thus the converging lines point clearly, as appears to me, to the creation of Man in one Zoological region and ~~ergo~~ therefore, of one variety; — since varieties are adaptations to the dissimilar conditions of different regions. But whether man is of one or more first parents in this one region and under this one variety, is a question science can not hardly answer. It must be met by a more thorough study of the laws of geographical distribution, in connection with general considerations derived from nature in her physical & spiritual totality.

The kind of development-theory which is rising to

beclouds the minds of those who look only downward for truth, that is into nature, is the Comtian or the pantheistic, either of which gives nature a plastic power to create or blossom out her new creations and asks for no lineal parentage for all germs. The Vestiges Theory is too gross for the ~~passages~~ metaphysics of the present age, and comes too completely within the domain of scientific research.

Such are some of the thoughts which your much valued letter has suggested. What say you?

As ever, most truly yours

James D. Dana

I. Cretaceous. — Reptiles & some whales, but no quadrupeds. — Plants see last number of Journal & preceding articles by Hayden —

II Tertiary

1. Claiborne Epoch or older Eocene. Various mollusc fossils, sharks, etc., but & no known quadrupeds.

2. Vicksburg Epoch or newer Eocene of Lyell (corresponds to what is called Lower Miocene on the Continent of Europe) — The Nebraska quadrupeds of the Mauvaises Terres, probably here. No fossil marine shells occur with the bones, and hence the doubt. The species according to Leidy are: of the genera of Multungulates Titanotherium (related to Cuvier's Palaeotherium of the early Eocene), Rhinoceros (2 species) & Ruminants Choeropotamus, Entelodon, Palaeochoerus, Leptochoerus: Ruminants, Oreodon, Agriochoerus, Poebrotherium, Leptomeryx, Leptauchenia, Protomeryx, Menycochoerus; Solidungulates or of the Horse family, Anchitherium (3 species); Carnivores, Hyaenodon, Amphicyon, Deinictis, & Machaerodus; also some Rodents.

3. Yorktown Epoch or Miocene of Geologists. — Some or all of the preceding species may have extended into this epoch.

4. Pliocene — About the upper Missouri & Platte especially in the region of the Niobrara, occurs a fauna later than the above & wholly distinct: It includes, among Ruminants, 3 species of Camel (Procamelus), 3 of Merycochoerus, 1 of Cervus, 1 of Rhinoceros, 1 of Mastodon, 1 of Elephas, 6 of Solidungulates among which 2 were of the modern genus Equus; of Carnivores, 1 of Felis, 4 of Canis, 1 of Arctomedon, 1 of Leptarctus. They are all different from

those of the post-tertiary.

II Post-tertiary.

1. Glacier Epoch – an epoch of Cold over the northern latitudes – the era of the drift (the lower a mostly unstratified drift). No bones are found in the drift.

2. Fluvial Epoch. An epoch of milder temperature – when the region of St Lawrence and Lake Champlain were submerged, and the sea consequently reached ~~stood~~ 500 feet above its present level; and when the northern portions were rising, generally as shown by the immense upper alluvial plains 50 to 300 feet above the present bed of the rivers, (the rivers much larger) was at a lower level than now – This was the epoch of the Elephas americanus & E. primigenius, the former from Kansas & New England south, the Cattle (the Siberian species) from Canada north. In Europe, the E. primigenius ranged from the parallel of 40° N to the Arctic coast of Siberia. Also the era of the Megalonyx, & Mylodon, (other allied species) as of the Bison latifrons, Cervus Americanus (as large as the Irish Elk of the same epoch) – a horse much like the modern horse but larger, Megatherium (in the southern States) – the Castoroides a gigantic beaver &c

3. Terrace Epoch – an Epoch of transition towards the present condition, bringing the continent up into (present level, and down into its present cool temperature). Some of the above species may have continued into it.

In Europe - during the Post-tertiary there were species of Elephants, Mastodon, Hippopotamus, Rhinoceros, Tiger, Lion, Hyenas, Bear, Stag, horse — mostly, many much larger than any existing species — in England. — The Rhinoceros (R. tichorinus) ranged as far north as the Elephant, that is to the arctic shores of Siberia where an undecomposed animals has been found of each in the ice near the mouth of the Lena.

In the Abhandlungen K. K. Geol. Reichsanstalt, Band II, 4to, you will find an article on the Eocene flora of the Tyrol (temperate) Climate of the Period; and another on the Miocene flora of Wien and the climate. If you have not the volume I can send it to you. There ought to be an abstract in the Journal.

Geology — I have written of something here — if not sufficient you must tell me. I believe our Continent has always been a considerably colder one than the European, perhaps with a greater difference formerly than now. But still there is no doubt of the greater warmth than the present in the post-tertiary after the Cylanic Epoch, and still greater in the Tertiary epochs. — Ever yours

James D Dana

(4)

thing precisely analogous to this
of course could not be expected in
inorganic nature. Yet when you
find Calcite taking the form of dogtooth
Spar in an extended region a rock,
as at Lockport, and the form of
hexagonal prisms in another region as
at Bonneville, have we not evidence
of external influences, of a region-kind,
producing varieties? — Were influences may reside
in the Constitution of the containing
rock. But whatever they may be, they
are extrinsic operating on the intrinsic;
and the results are permanent varieties too; (or
there is no change without subjecting
them to wholly new influences.

It is true that it is not easy to
distinguish varieties — or to decide whether
what we call species may not be varieties.
Here is the great region of research. In any
case, we have to assume that the
original units are the species, and forward
there direct our search, or we have no
definite aim in view.

I think Agassiz is going to run aground
with his principles about grades of subdivisions in classification.

Here is an easy possibility that will not be on the restraint. — Many searchers
& thanks for kind words; & answer your
questions — I should
and have read —
Cambridge — I should
judgment
In Glacier
Lake
Now August. 13 at
Mr Pratt
shall be
Lawrence
Allen
Kind

Agassiz

{Your MS. has been
rec'd — two packages

New Haven, Dec. 4,
1857

I thank you for
letter containing further
suggestions & thoughts on the subject of
Species. I have been trying ever
since it was rec'd to find time for a
reply. But my geological lectures
and recitations give me so constant
employment that I have not found
the leisure moment until now; and
the present moment is not a long one.
I am very glad to have your
State difficulties & objections, both
from the improvement to myself &
the opportunity to give additional
explanations. ——— I think
that the points you bring out —
in your letter deserve strength

in your own mind, from the idea with which you start, — that organic species are more variable than inorganic. Among the inorganic there is an invariable number as to chemical forms. But the variations in the organic are in form + color mainly; and in the inorganic the variations are great in these respects, as great as in the organic — I had always thought greater. The difference is this, that the inorganic is without diversity of parts, and the variations of form all lie in one single range. Set the species Calcite (carbonate of lime) against any plant or animal species; its crystals are exceedingly various, sometimes hexagonal prisms like beryl sometimes in prisms with pyramids, + so on through hundreds of distinct forms. Then consider the diversity among fibres + granular parasites, diversity of color

and texture. This is no solitary instance, but a fair example for all, at least as regards the great principle. It appears to me that the Analogy as regards variation is perfect. It is as true of an element, as oxygen or Carbon, as of a compound. My principle is that variation is part of the law of a species, and the law, or extent + range, of variation a subject to be thoroughly investigated before the species is understood. — Again you say rightly that animals + plants when domesticated tend to run into races — or, in other words, domesticated animals, — that is the species capable of domestication, have this tendency, — the transplanting from the original locality ending in this result. Some external influence is here concerned in determining these races or permanent varieties. Any

New Haven Aug 11, 1858

Dear Gray —

I like your exposition of the Newcom case, and hope the discussion has reached its end.

The article on tendrils I have no recollection of — Send it again, and I will see it go in; and if you can, I wish you would send it by return mail. The article on heights by Buckley slipped out of sight, and I cannot find it. Can you not send that again; for I intended to have inserted it, thinking it of much interest. —

As to tertiary climates for America we know little. The Pliocene shells on the U. S. Coast appear to show that the climate, or temperature rather of the waters

(4)

in the Eocene had a flora which was prominently Australian & Von Ettingshausen makes the mean temperature of the year there between 18° & 22° R. — The Post tertiary of Europe, after the glacial epoch, appears also to have been warmer relatively than that of America. I think that N. America from the Cretaceous on had a much longer amount of high temperate or arctic land & of water, and hence was cooler, than now, our region that are under the same isotherms at the present day. —

Ever sincerely yours
James D. Dana

Prof. Gray —

at Charleston S.C. in the Pliocene, was near that now existing there, and not warmer, I think, than that of the parallel of 30° (northern Florida) — The shells of Virginia & New Jersey would make about the same difference — But the Continent was at a little lower level then — there was no Cape Hatteras, — and the Gulf Stream would have warmed all the waters south of Cape Cod. — "As for" the land climate, we have no data. There is no Tertiary of any kind north of Cape Cod. In the Pliocene in Nebraska there are remains of Camels, which indicate seemingly a warmer climate there; but perhaps not a greater degree than that inferred from the shells of the Coast. We do not

know whether they were warm temperate species or not. Yet as they accompanied remains of Rhinoceros etc. the probability is that their climate was considerably warmer then now.

after the Glacial Epoch — there was a Period of warmer climate again and of northern submergence, when the post tertiary deposits of the coast of Maine, lake Champlain & the St Lawrence, and regions further north were formed; and this was the era of the Mastodon & Elephant of our Continent. the mean temperature may have been then about the same as now, but the seasonal extremes less owing to the northern submergence.

The fossil plants obtained by Newberry, Meek & Hayden from the Cretaceous of the Rocky Mountains seem imply a colder climate then at that time than in Europe; and if this was true for the Cretaceous, it was probably (they imply) so also for the tertiary — that is the difference between Europe in climate that America was relatively greater then than now. Colder than Europe, Austria (Tyrol)

My dear Gray: —

I have rearranged your geological part to suit what I believe to be the state of the case & make the most of the argument. — I presume that the Kentucky post-pliocene is more recent than the drift, that is, is fluvial: you will see how I have managed it: arranging it as if it were so, but mentioning Lesquereux's opinion. —

Your argument is a good one. On the main point — whether the same species

has been excited in
more than my country,
I have not been able
to satisfy myself.
Agassiz does not allow
near enough to migration,
& I question a little whether
you do not allow too
much, and the true
solution of the various
difficulties is between you
You make out a strong
case for your side.

It does not seem to me
that Darwin makes out much
in his paper. As an article
on the variations of species

it is good; but when it is turned into an argument on the origin of species it is in my view exceedingly defective. When his variations have gone so far that mixt lines produce only infertile hybrids, then I would admit that he has touched the subject of the origin of species; but not till then. —

In haste

very truly Yours

James D Dana

I changed with regard to the minimum number of years in the present era" - because the present era means nothing special unless restricted to the period since the creation of man; and with regard to an minimum period since that time it is not the place to ~~present~~ make any statements, as geology has nothing definite to say on the subject.

I ~~changed~~ changed paragraph about the ~~the~~ megatherium, mylodon etc. of the post tertiary of N. S. because there have mostly Southern animals and gave no definite information as to the climate of the period.

You must change my writing condense it as you think best.

A copy of Lesquereux's article will be mailed to you today

New Haven, Mar. 9. 1859

Dear Gray:—

I am very anxious to have your revision of what I have written in concluding my review of Agassiz. It relates to Classification. As we have been compared notes on the subject, I cannot reasonably ~~expect~~ that you will accord with me in my conclusions. But I do not wish to err in misunderstanding or misinterpreting Agassiz. Neither do I wish to commit any obvious blunder myself. Moreover I wish to express myself freely without producing any irritation on Agassiz's part. Now you are an excellent judge in all

these matters, and I ask
for the benefit of your judgment.
Please make your criticisms
freely – and you will greatly
oblige Yours most faithfully
James D. Dana

My kind regards to Mr
Eaton, who I suppose is now
with you – Mrs Dana ~~fondly~~
her love to Mrs Gray. –
Yours J. D. D.

Please return the proof
after keeping it a day, as the
printers are in a hurry to go on
with the printing.

(1

New Haven, June 14, 1859

My dear Gray: —

I am very greatly indebted to you for your letters on the Marcou affair, both of which came today. This morning after receiving the first of the two, I wrote as follows to Agassiz: (I had seen the pamphlet, B.S. Jr. having rec'd one from Marcou by mail): —

Dear A. —

I find by a copy of a pamphlet by Marcou, which has just been received here, that Marcou, through some means, has got hold of our confidential correspondence, and charges me with refusing to publish your paper

in case I publish the closing part of mine in reply. I am sorry

without scirroing, and with yielding only upon you threatening to withdraw your name from the Comm of the Journal. I hope you will do all you can to correct this error publicly. You know that in my correspondence I at first objected strongly to your views, and said that if you would read the book you would be convinced that I was right; and in my last letter, I based my objections to publishing (I made no refusal) on the ground that you had not read (in new Testament) the book. When you were here, you told me that if you had read it before publishing, you would not have published at all. I presume you have not seen the paragraph in Marcon's pamphlet in which he makes this development. I do not wish to write any thing further on the

subject, and so leave it with you. — With best wishes to

J. D. Dana

I don't know but A. is already N, so that my letter will not reach him. I may have a letter from him, in consequence of your prompting. If nothing comes, I shall have to follow your advice, and make a brief correction of the false statement. So much as this is true, that Agassiz with me finally that if I did not choose to publish his paper I might take his name from the cover. I wrote in reply that I would publish it, if still desired, — but for I had not refused it; but that I still thought it due to me and to science that he should read the book before publishing. —

Your copy of the Marcou pamphlet has come; and as Silliman has me, I will return it to you if you wish it again. If agassiz prefers, to befriend Marcou, whom he acknowledges to B. S. Jr. was unprincipled, to dealing fairly with me, if there is a lack of something in him. — I suppose there was no intentional wrong. But such double dealing is sure to betray itself & prove a double failure. —

I wish you would send those corrections of the Eulogy, as soon as you can, as mercury not. get them from Prof. Felton. It is already printed off: but we can insert in the Errata (or in a paragraph in the Miscellany, if that is allowable under the circumstances.)

Marcou makes much of what he considers errors in my history of

American Geology. — In the first place, I have written no formal history; secondly, in what I wrote I spoke of the New York Geologists in a body & did not omit them. I overlooked Vanuxem's part in the discovery (or anticipation) of the Cretaceous. & that is the only oversight. — But I shall make no reply on this point or any thing else in the pamphlet, beyond what you have advised. —

Sincerely your friend
James D. Dana

Did you hear anything when in New York from Dr Torrey about my being urged because it was proposed to give me a partial salary for a partial course in Columbia College? There was some foundation for the statement though it is an exaggerated expression of my feelings. Moreover, I am confident neither Dr Torrey or any one else except Mr Allen knows all the facts. — I give you the facts on another leaf — and if you have heard from Dr Torrey anything about it, you may (without mentioning it as my suggestion) send him the leaf — for I wish him to understand that I have not been unreasonable & [illegible] the need of [illegible] writings.

Exp. Except told me that Dr Torrey rather resented me as having taken [illegible] at Mr Allen's proposition.
or ill-tempered. —

My article on Bushnell — was intended for clergymen; and if you know the ignorance among them, & the readiness there is to take in Dr Bushnell's rhetoric as [illegible] truth, you would [illegible]

New Haven, June 20. 1857

My dear Gray:—

I enclose what I have written for the Journal about Marcou. If you think best, I will suppress it altogether, as your note just received suggests. I did have a note from Agassiz: but there is only an allusion to the pamphlet. I enclose it that you may see its tenor, and understand the exact condition of things. Please return it, and give me your opinion about publishing by return mail if possible, as our printers are ready to print the signature.

I am glad you approve of

not agree with you, & should still consider the Lebanon Springs Meeting of the A. A. A. S. as the best point of departure in Journal

(4)

schemes, in the drift period, and it could not injure at all your argument. Having Rhinoceroses at the Lena in Asia and Elephants near Behring's Straits, it is obvious that the northern hemisphere must have been much warmer then now, for America as well as Asia.

If I have not reached the point you enquire about write again—

Yours as ever

James D. Dana

P.S. Gray

You spoke of a review of Hooker for the Journal. Whatever you send is valuable. If I should

my Columbia College
action.

I have looked over your
paper. There is nothing there
about which there is doubt
which affects your conclusion.
I am not certain as to how
far the remains of the Elephas
primigenius have been found in
N. America: but in Asia they
are found on the borders of
the Arctic ocean, near Behrings
Straits &, at the mouth of the Lena.
Some of our geologists regard the
Glacial epoch as one of
submergence: but the epoch
of the St Lawrence & Lake
Champlain deposits, would
Still be, one of just the degree
of submergence you state.—

with the same condition of the
climate: for the inference with
regard to the climate is drawn from
the fossils that belong to the
epoch next following the drift.
The unstratified or true drift
according to all authors preceded
the stratified (or fluvial) drift
a deposits — and this is
geological evidence of the
best kind that the glacial epoch
preceded the fluvial. The only
point in which I differ from
Hitchcock & some others is that
they appeal to icebergs during
a period of submergence, and
(with Agassiz etc.) to glaciers
during a period of somewhat greater
elevation. The submergence
would imply a still warmer
climate, or at least milder

of England in this epoch, the Elephants & Rhinoceroses about the mouths of the Lena in Siberia and towards Behring's Straits when the Elephants tusks are now so abundant that they are exported in large quantities, all prove a much milder climate than the present — an effect attributable solely I should say to the diminished extent of Arctic land.

The Aleutian Isls were probably in a temperate region then, as New England is now. It is true the Elephant of Siberia was a hairy one. But no animals of the kind, having or not having hairy coats now live beyond the tropics and the fauna of British and Arctic Siberia is

The third epoch of the Post-tertiary is that of the elevation of the Continent

* It is meant here, during the Miocene & Pliocene periods, the Lemurs & allied races abounded in our Northern valleys. —

2 Mainy Square Ithaca
Sept 4 1858

My dear Gray: —

I regard the Post-tertiary Period as divided into three epochs — that of the Drift or the Glacial epoch, an epoch beyond doubt of greater cold than now, and therefore, as well as for other reasons, of more elevated land over the higher temperate latitudes & the Arctic — Then 2nd, the epoch of Northern Subsidence which I have called the Champlain epoch, and, might very

As ever sincerely yours
Jan. 19. 1859

appropriately be called the Champlain Epoch – as it was the era of the formation of the upper alluvial plains along our rivers, and also as well as the higher sea beaches, or sea shore formations on the coast of Maine, & the borders of Lake Champlain and the St Lawrence. The facts indicate that the land over the north (north of New Jersey) was much below the present level – 60 to 200 feet along the coast of Maine – 300 to 400 Connecticut, 400 Lake Champlain – 500 St Lawrence near Montreal; and the upper terrace or flat on the Great Lakes, as well as that of the rivers over this northern States belongs to the

same period. You remember that besides sea shells and sponge, the bones of a whale have been found on the borders of Lake Champlain. In the Arctic there are elevated sea beach or seashore terraces several hundred feet above the sea. There is no doubt moreover that it was a epoch of warmer climate than now. The shells in the St Lawrence post-tertiary deposits do not indicate it; and this was not to be expected. They are all cold water species. But as the water has a temperature above 32°, there might be great changes of climate over the land, which now sink down to 40° below zero without affecting the temp. of the Labrador Current. The lions, tigers, Hyenas, bears, Elephants, Rhinoceros &c

and will write him
next week. —

Sincerely yours
James D. Dana

Prof. Asa Gray

New Haven, Nov. 28. 1862

My dear Gray: —

You have
heard already that the Geology
is through with — Yesterday was
Thanksgiving day for that
& other causes of joy. —
I am not beyond being
fatigued rather easily with
work. but still I am
going to try work in the
Perpetual way next
week, my geological
recitations commencing
on Monday. —

A copy of the Geology for your acceptance is already on its way to you, and I hope you may receive it this P.M. — It makes a larger volume than I had intended: but by means of the Synopsis in the Appendix (App. I) it may become a small work to College Students.

I intend to look at the formal revises from this time, if I do not find it too much in connection with College duties. I have groaned inwardly over typographicals for a long time past; and have wished to do something towards preventing them. But — as long as I had the Geology in the works, I made it a point of duty, not to look at a page of formal proof: and previous to that, I was in no condition to think of it. —

I have sent Darwin a copy of the volume.

although the blanks may still be large. –

But I need not write more, for you understand the subject quite as well as I do, and were ahead of me in reaching right views.

Very heartily yours
James D. Dana

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Address **DANA & SILLIMAN,** New Haven, Ct.

New Haven, June 22 1874

My dear Gray:

Last week was a busy week here, and I therefore left your note unanswered.

Dawson's remarks on the warm water flora in the Gulf of St Lawrence, occupying the first third of his address are right & properly presented. The "wonderful" lesson from the facts as to the fixity of species" may be questioned. His other lesson about climate is wrong; for he makes the warmth of the waters along the coast & Labrador as to warmth

of climate over the land. He attributes the warm current along the coast to an elevation of the land, thus throwing the Labrador current several degrees east of its present route. (The explanation is mine, and I think a good one; for a rise of but 200 feet would close up the straits of Belle Isle & put the Labrador current far east of Newfoundland) But such an elevation of the land would have been attended by a cooling of the climate over the land, simultaneously with the warming of the coast-waters. His remark about Europe, and Rep. Geikie bear, therefore, no foundation - not the "slenderest possible". -

His statement about the abrupt appearance of new forms in the "record of the rocks", is right, as Darwin and all writers on evolution acknowledge. But his method of using the fact should bring him into "contempt among all thinking men". The citation he makes from an opponent of Barande is open to objection, because the suppositions it contains are mere suppositions, and the blanks in "the record" are scarcely lessened by them. But it is not deserving of the contemptuous condemnation which Dawson pronounces, with a masterly abuse of Logic and of the language of the author he combats. There is no question that geological discovery has diminished many of the blanks between groups; and this alone is enough to take the starch out of the reductio argument of geology.

New Haven, Aug. 10. 1876

My dear Gray:

Your note and book came in this morning. I am glad you have brought your papers on Darwinism together, and I thank you for the copy you have sent me. You have always written judiciously on the subject and have nothing to take back, which is much to say or to have said of you. — I am inclined to insert your last & new chapter in our October Number: What say you. — You bring out well the fact that Darwinism

at the dinner, or better some hours earlier, and that you & your wife come and stay with us over Sunday. I am going with Mrs Dana to New York Monday morning (I, on an excursion to Long Island for geological purposes) so that we will accompany you that far on your way.

I hope you may not be able to carry out this plan. We hope, Mrs Dana & your friend, that we shall see you both for a longer visit on your way back from the South.

Mrs Dana sends her love to Mrs Gray. —

As ever Yours

James D. Dana

does not account for variation. It is that which has made me feel that Darwin's work ought to have been styled — On the connection of Natural Selection with the origin of Species — Not the origin of Species, by it, which the title seems to imply. — "The" survival of the fittest" hits it off admirably for that it is; and this means that it treats of the methods of extinguishing the unfit. Perhaps the destruction of the unfit would be better, as it is more precise definition of the principle. I suppose there may be some results of this selection analogous to what Man uses in improving breeds; but it seems to me that nature does little of that work. There is variation & improvement through environment conditions & inner unknown workings, and then a whittling away, or a leaving to decay, of what is unfitting. I did not intend to scribble on this way when I began.

Prof. Marsh & Huxley are having a grand time over the bones — The birds with teeth, and the Pleiosaurs without teeth, etc, etc. He told me that he was going to invite you to a supper (or dinner) on Saturday evening, with Huxley and a few others, not over a dozen in all — and no speechifying! — I hope you will come. And I further hope that, as you are going Southward you will make your start with Mrs Gray in time to be here

New Haven
March 15th 1878

My dear Gray; — In the Miocene there was an extended submergence of Arctic lands, as we know from the Miocene deposits. The depth of the submergence I have not seen stated — I suppose not great. The height of the Miocene deposits would determine it, and no account of these which I have met with gives it. [Perhaps 200-300 feet?]

The evidence of land communication at this time between America and Europe rests wholly on the identity of American and European Miocene, or later, plants & animals. I regard the evidence as good. —

(4)
cannot say; but it is altogether probable that in this era of submergence the Caspian and Aral were connected with the Arctic. It is admitted by the Swedish Geologists that the Baltic communicated with the White Sea by a channel leading northward over Finland. Northern Siberia experienced the mild climate (probably a warm or at least cool temperate ~~climate~~ climate), as the herds of hairy elephants — or rather their remains — prove.

As to the depression of northern Siberia, we have no facts. It is an immense alluvial region without boulders; and it is my opinion that the deposits were made chiefly by the ~~flood~~ floods, streams during the melting of the ice of the Altai Mts, as a sequel to the Glacial era.

As to Europe being a peninsula since inhabited by Seguoia &c:- This was a fact, in that era of submergence if the Caspian-and-Aral region were then connected by salt water with the Arctic seas. But it was probably not true in the Pliocene & Miocene. In the Eocene, Europe was a group of large islands.

The region between Scandinavia, Iceland, and Greenland I believe, or think, was under water as now; for if not, the Gulf Stream could not have flowed into the Arctic Seas and contributed to their warmth (helping thus a Pacific tropical current, or "gulf stream" flowing northward over the submerged regions of Behrings strait?): but there may have been a connection between Asia & America directly across the polar regions, or any where that would not have interfered with the Gulf Stream.

I come now to your question as to the lands north of us being higher in the Glacial era. The evidence is pretty strong that the land was higher; and it is probable that there was land communication across from Europe to America; and there may have been migration between the continents before the cold had fully set in; and had made the way impassable. (is generally admitted or assumed. That there was such migration)

I think that the cold of the Glacial era was due to a shallowing of the ocean between Britain, Iceland and Greenland, so that the Gulf stream was thus excluded from the (3) Arctic regions.

Then, secondly, as to subsidence since: The fact of a lower level of the land is admitted by all, (among geologists) the only difference being that some speak of the subsidence as having been begun in the Glacial era. It is proved by the existence of beds of recent shells in various arctic lands at heights of a thousand feet and less; and even 1800 feet at Polaris bay, Greenland; of similar beds on the coast of Labrador at a height of five hundred feet, and probably, says Packard, of 800 feet; near Montreal at a height of 470 feet; on the coast of Maine to a height of 217 feet, etc.

I have held the view that this subsidence of the northern regions brought on a mild climate and so promoted the melting of the ice. The remains of Mammalian life found in Britain and in northern Europe and Asia, as well as in northern America are proofs of a warmer climate.

as to Sahara and lower Egypt I

a few geologists still hold to the view that the boulders of Europe were distributed by ice bergs, and that the northern part of Europe, down to latitude 50°, (excepting the Scandinavian mountains) was under salt water. Thus they make the submergence, spoken of above, a very much greater one than is sustained by facts. I do not believe in such a marine submergence when there are no marine fossils, or other relics, to indicate it. and this would not make a peninsula of the continent, as it would only ~~cause it~~ contract it on the north.

In North America the ice sheet extended about to the parallel of ~~40°~~ 39°, and it reached west of the Mississippi to western Iowa. West of this, over the great interior of the Continent to the Sierra Nevada and Cascade Mts, the evidences of the great northern glacier fail, owing undoubtedly to the dryness of the climate and the warmth of the summer. There were however extensive local glaciers about the summits of the Rocky Mts in the glacial era. ——— If I

Nov 15 70.

(6) have not been sufficiently explicit in my answers to your questions, you must ask again.

My wife has been my amanuensis again, as I am not yet in satisfactory condition, although going on with my College duties. —

Sincerely yours
James D. Dana

New Haven, May 13. 1878.

My dear Gray

My knowledge of Dickering began with his appointment to the Expl. Expd.; of his earlier life I know nothing beyond what the family obituary notice states.

The appointment was received by him during the close of the year 1836, when the projected expedition had Commodore Paterson at its head — probably in December, the date of mine; and in August of 1838 the Expedition sailed under Capt. Wilkes. And during the cruise we were not much of the time together — he in the Vincennes, under Capt. Wilkes, and mine the Peacock, under Capt. Hudson, were often at different ports or islands or a group, when not in different regions. Hence my personal knowledge of incidents in his Expedition life is small. —

When he joined the Expedition

he had already an extended knowledge of Botany, and of Zoology in all its departments. He is special delight for study from that of Birds. But he collected & took notes in all. No one was more energetic in Botany or made larger collections of Botanical facts. — I was going to say, 2 specimens; I but while a great collector, I assume no horticulturist. Beechey, who made excursions especially for the purpose, may have exceeded him in this respect. He was of course necessarily the earliest moment possible after a port was reached, and where not nothing in any branch of natural history seemed to escape his attention. — Some of his longer excursions were the following

To Pico Ruivo, Madeira (Wilkes Narrative i, 13

Organ Mts, Rio Janeiro — i, 69

Chilian Andes, the snow limit near Santiago where we [illegible] two days in the mountains, & spent a night, under the same pair of blankets — he having had alpine plants especially in view i —

Cerro de la Campana Andes, p. 252, where he encountered a Condor & got out his pocket knife for defence p. 265

The mountains of Tahiti.

Ascent of Mauna Loa, Mauna Kea and other mountains of Hawaii, also those of Maui, & other Hawaiian Islands. — iv, p. 60, 81, 143, 198, 252 etc. etc.

On the return of the expedition we supposed that he would go on with the descriptions of the Birds. But he had no ambition to describe new species; and he thinks he did not like the responsibility of it. His studies while on the expedition & at home began to appear, had had reference especially to the geographical distribution of plants and animals: and he continued his investigations in this line ever afterward — as you know.

I have never had any correspondence with Dr Pickering since his return, and really know little of his movements.

Very truly yours
James D. Dana

Saturday, Mar. 16 78

My dear Gray:

I add a word to my letter of yesterday. – The chief evidence of the Connection of the Carpini with the Arctic – that which I had in mind – is formal: the occurrence of arctic species in the Carpini. – See on this point Jour. Sci. III, xi, 500. – The author there speaks of an earlier Connection with the Back sea, when that was fresh, which is possible, though not proved. – The connection of the Carpini to Arctic seas supposes a submergence of at least the western part of the Siberian steppe. But I do not remember to have seen any account of

Marine shell beds on the slopes proving
submergence, and most writers make
the beds wholly of freshwater origin.
But there is evidence found
in favor of the submergence of the
Lake Baikal region; which, if of
value, would carry with it an
immense area.

Very truly yours
James D. Dana

New Haven, July 6. 1878

My dear Gray:

I received your proof this morning. Your views are excellent and well brought out — a sensible view of the Prairie question. I never thought much of Whitney's assumptions.

I am glad you have had a pleasant time with Dawson. He is an ardent worker, though he does not always hit right in his conclusions. His views on the Glacial

the life of the sea would have travelled far inland, and its remains would have been sufficient to establish approximately the amount of elevation. all New England down 3 or 400 feet or more, and sea-cliffs only to a height of 300 feet on the coast of Maine would hardly be a possibility. Then again we can measure the depth of the submergence by the coast deposits & terraces along Southern New England and ~~deposits up the river valleys and find that the streams~~ in the shape of valley terraces; and so prove the submergence to have been small, and the river-valley terraces to have been made by great streams occupying the valleys — just such floods

era ~~head~~ formerly - many adherents;
now very few. I do not
know any one on this
side of the Atlantic that
agrees with him excepting
his son, and he has diverged
quite a way, making much
more use of glaciers than
Dawson used to do. The
one great trouble with
Dawson's theory - that is
the submergence theory,
is that there is no evidence
of it in marine relics over
the submerged land, while
freshwater relics are not
uncommon. We find such
marine relics up to a height of

500 feet on the St Lawrence;
only to 200 on the coast of Maine;
and we have to admit
submergence accordingly.
But if the submergence was
one of two three or five
thousand feet there should
be this kind of evidence over
50 vast a continent somewhere
up to that height - or at least
above 500 feet. When sea-
waters flow in over a submerged
land sea-life will necessarily
go in too - fishes first, and
then the slower movers.
Supposing the submergence were
only for one century - not a
hundredth of the time any
geologist would think probable -

streams, as would have been made by the melting of the ice of centuries.

There is a drift toward the theory of submergence on the part of some just now setting in again, as you will see on reading Stevenson's article in p. 245 + beyond of our last volume. The high flat terraces of so great height are a great puzzle - I admit, especially as they look down toward the Ohio Valley, and there seem to be no chance for any obstacle to have existed that could have set the

freshwater back to such an
altitude. But there is that
difficulty – the absence of
every trace of marine relics.
Lesley, in our last no., seems
to support Stevenson's view;
and yet he mentions a
fact – that about extensive
clayey deposits with leaves
in abundance, order such a climate
which goes
right athwart it. Damming
by ice may meet some of
the cases, even the hardest.
We need more light;
but at present, the facts
favor freshwater submergence
not marine. – I had – better a few days since from Lesley in which – he admits he does not know which side to take.

Very truly yours
James D. Dana

Scandinavian, etc. etc.

My view is — and it is sustained by facts abroad — that — the Glacier period may have been on a somewhat greater elevation to the north. Gen. Warren has really shown that formerly L. Winnipeg waters flowed south into the Mississippi, and he explained the fact by the hypothesis that the region was formerly higher than now so as to turn the waters S.; and that afterward there was a sinking to the north that set them flowing northward to Hudson Bay; thus confirming the idea of an elevation N.; without having had any reference to the views of geologists. +

Whatever was the conditions in the Glacial era as to elevation there is no doubt of the submergence below the present level in the

New Haven July 13. 1878

My dear Gray:

Dr Dawson flatters himself that the Icebergists are the great ascendant majority. In fact he overlooks. — Hitchcock in his 6t Rept sustained the Iceberg Theory — in his N.H. Rept (last out) he + Upham speak only of the Glacier theory + river action. — Shaler believes, takes the same ground. I spoke of Stevenson in Pa. as drifting the other way, that is, to maine submergence; but his submergence to 2600 feet + over, was post glacial. The recent Wisconsin, Iowa, + Minnesota Geol. Repts, adopt the Glacier theory. Newberry in his Ohio Rept has both mixed; the submergence + the great area south of the drift and the moraine system, & Glacier era

Dana

being made marine, although no trace of anything marine is mentioned; whether he now holds to this view — which is about the same as Stevenson's — I cannot say. In fact I do not know of any recent report or paper excepting Dawson that adopts the ant-and-out-Iceberg theory of the drift. The universality of the Scratches over New England is proof enough against it; for Icebergs could not have Scratched through the earth + gravel that — owing to their droppings — we have covered the sea-bottom, except at occasional points; and the Scratches they could make would have had a course dependent on the general oceanic current — say the Labrador current, and could

not have followed every mountain-valley as did the bottom of the glacier. If we take the anomaly of the submergence after the Glacial era (in what I call the Champlain period, it commencing with the submergence and the melting of the glacier) from the Marine Shell deposits along the coasts, we have a concordant series of facts, we have for the depths of the submergence as compared with the present level. 85 feet in Nantucket; 75-100 at Pt Shirley, near Boston; 217 in some places on the coast of Maine; 470 feet to 500 near Montreal; 500 to 600 on the Labrador Coast; 1000 or so in the Arctic.

Lyell sustained the Glacier theory; so do most English Geologists; and all the

July 13 1878

[illegible] following, when the glacier melted (owing I think in part at least to the warmer climate occasioned by the lower level of the land to the north,) and made tremendous floods along all the river valleys, and so formed the high valley deposits & terraces. The sea-border formations make the submergence along southern New England probably not over 30 feet; and then, northward, its depth is indicated by the height of the sea-shell deposits as above mentioned.

After the era of submergence — which was the era also of the Mastodon, Siberian Elephant etc — though there may have appeared before & Champlain species be also of Glacial existence in non-Glacial latitudes — came the elevation of the submerged

land & their present levels.
I do not mean to imply
that the whole Arctic was
submerged 1000 feet; for the
facts are not sufficient to prove
this; but that some parts were.

If the shallow seas between
Scandinavia, Great Britain,
Iceland & Greenland, were
shallowed to a few scores
of feet, the Gulf Stream would
have been shut out of the
Arctic, as it is now at Behring
Straits; and this would have caused
the Glacial climate? There may
have been low land connecting
Asia with Arctic America in
some parts of the Polar regions.
This is not incompatible with
existing facts, nor with the views
I would advocate. ——

During the Glacial era there were
local glaciers in the Rocky Mountain region
but none over the interior of the Continent
west of Lower & Lake Winnipeg, owing to

Amherst, Aug. 10. 1878.

My dear Gray:

I have read your proof this morning, and like well your ideas. — Only one query I make — Why do you speak as you do of the conflicting views of glacialists, when, if the Glacier theory is not true and the Iceberg theory — the only other — is, all your conclusions and reasoning come to nothing. The Iceberg submergence would sweep off your fruits — not drive the Tertiary South, and that would be the end of them — a or pretty nearly. One all-prevailing argument against icebergs is this — that icebergs bring rocks that were once glacier scourings away from emerged land; while in New England there would have been on that theory but two or three — or a few at-

Present Flora of Europe, in its Post distribution from Arctic Miocene (Glacial) Tertiary Centres. Compare the Canadian Flora. Consult Lyell's Antiquity of Man, and Climate and Time. Consult Dana's Manual. Consult Darwin's Origin of Species. Consult Wallace's Island Life. Consult Asa Gray's Memoirs. Consult Dawson's Canadian Naturalist. Consult Agassiz's Geological Sketches. Consult Hooker's Flora of New Zealand. Consult Hooker's Flora of Tasmania. Consult Hooker's Flora of Australia. Consult Hooker's Flora of India. Consult Hooker's Flora of Antarctic Regions. Consult Hooker's Flora of Himalaya. Consult Hooker's Flora of Japan. Consult Hooker's Flora of China. Consult Hooker's Flora of Africa. Consult Hooker's Flora of South America. Consult Hooker's Flora of North America. Consult Hooker's Flora of Europe. Consult Hooker's Flora of Arctic Regions. Consult Hooker's Flora of Antarctic Regions.

the most emerged peaks; and
the drift has generally travelled
not over 500 miles, much it
is 10 or less and belongs wholly came from
hills that would have been in the
bottom of the arctic iceberg Sea.
A huge boulder of 1000 tons near
this place & hundreds smaller were
brought from the high ridge of the
Connecticut valley, which would
have been imprinted with icebergs
in a sea deep enough to float
the loaded bergs. The drift-
Scratches follow pretty closely the
valleys, and the Striations are
universal in nature is a possibility
with icebergs. — You must fall
in with the Glacier theory if
You wish a foundation for your
views; and the facts you present
are the best of proof of the correctness
of the theory. —
One point; you do not in your closing part
allude to the climate of Europe
as a cause of peculiarities in the

New Haven, Aug. 14. 1878

My dear Gray: —

I did not intend to say that the Glacier many always followed the courses of the valleys. But that it under surface did so to a very large extent. All over New England the scratches in valleys follow the courses of the valleys. They do so, in the Connecticut valley, all the way to New Haven, and they do so along the small streams over the 8 to 10 miles west of New Haven. At the same time over the higher parts of New England, as over Litchfield Hill, etc. in western Connecticut, and other parts of the valleys, and about the higher regions of the mountains, the

(2)

Peraticks have a SSE to S. E. Course.
Boulders in great numbers upon
west side of the Connecticut valley
and west of New Haven which
were carried out of the Connecticut valley SSW
by the bays, bays & sounds [illegible] of the valley.
- they being
At the same time there are
also boulders from the Northwest
that is, those that were carried to the SE
or SSE. There are find limestone (marble)
boulders from Canaan in Northern
Connecticut that were lying in the drift
within three
miles of New Haven & so on.
and there are multitudes of boulders all [illegible] Island
that way coming [illegible] a SSE course, as the rocks there
When the mass of the ice had
a movement dependent on the
slope of its upper surface (as in
all fluid movement), while the
bottom of the ice, lying in the
valleys, (grooving the surface) was
followed the valleys. - just
as it would be with pitch.
The thickness which has been attributed to
Comes from the accumulation of

3 Aug 14 78

glaciers & Catskills or Mountains
— as on the top of Mt. Mansfield
a region I have examined; and on
the White Mts. But the
glacier theory is not affected by the
question of thickness of ice.

Shaler I do not understand.
From his Memoir in Mem. Boston
N.H.S. printed in 1874. You would
think him head over head in the
glacier theory; for he makes the
polar ice cap thick enough
and extended enough to cause
a sinking of the earth's crust
and a submergence in part of
all the way from the Pole to New Haven
the land. The glaciers must
have been long growing to
attain such thickness and
dimensions; so that the era of the
growing ice was long before any
submergence began. (The era
of submergence I call the
Champlain period.) & never
was so much of a

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Glacierist or Shaler's theory
maker of him. In another
article in the Proceedings
presented at the meeting in
January 1895 he endorses again
this theory with regard to
accumulating ice over the
northern regions causing a
subsidence; but there he
puts his ice in before the Miocene
period & makes it cause a
subsidence
of the Alaskan region
such as would let in the
warm Japan current by the
Pacific. & Then he accounts for
the warm Miocene climate
in a way
is done in my Geology (pp. 754-756) &
know of no fact or argument
the idea of ice-accumulation
before the Miocene, and I am
sure he has none: but he shows
himself to be a strong glacierist
strongly
& this is reasonable.

(* Hunt on the cold of the Glacial climate, at variance with Dawson's view, and also the Shaler theory of the Glacial Epoch. Hunt (Dawson) thought to be caused by snow and ice, the Gulf Stream & all kept open from the arctic by elevation of the land between Greenland & Scandinavia.)

You will ask whether he gives
Credit to the Geology with regard
to that idea as to miocene
climate – brought out by him 6 months
after the book was published
He does in a partial way. He
evidently did not derive his
theory from the geology; for
he says, in the remark that
whole his paper was going through
the press he found a reference
to Mr Bradley's view on
the subject advocating such a
theory. My geology does not
give a reference to Mr
Bradley's views, but presents
the theory on the Chapter on
Changes in climate as the
most probable one both for the warmth of the miocene and the cold of the Glacial era
Shows its efficiency by reference
to Croll's calculations with

* Mr Bradley aided me in part greatly when my Geology was in the press, and was at my Elbow when we were discussing the Glacial Climate + the Gulf Stream theory. Although I had begun the Geology before I knew Bradley's views.

~~regard~~ reference to the heat carried (Aug 14 '78.)
in by the Gulf Stream, just as
Shaler does. ~~He~~

Shaler advocates ~~the~~
his theory that an ice cap
may cause submergence also
in an art. read before the
B.M.H.S. in Dec. 1874; and (Proc. p. 200)
here the subsidence spoken of
is not a general continental
submergence, such as iceberg
would require (2000 feet &
upwards for New England);
but, a border region, & such as
only the submergence of
one as I and others admit to
have been submerged, seashells
proving it.

~~Are Shaler became~~

Are you not wrong in saying
that Shaler supposes the Glacial
era one of submergence of Icebergs, a la
Dawson!

Very truly yours
James D. Dana

New Haven
July 18. 1880

J. D. Dana

My dear Gray:

Your proof has come, and I have read it. and am glad you propose to give us more from DeCandolle's excellent work. Your notice is a very valuable review of principles, good for our readers, and we will insert the remainder in our next with great pleasure.

But this going abroad I had heard nothing of it.

When will you spend your time? I presume in England as that is the great centre of Botanical herbaria libraries, gardens & men. I hope that you will still send us from time to time notices for the Journ. Sci. —

I am off on a Geol. Exc. this morning and close hastily with my wishes for your success & happiness abroad and the same too for

Your excellent wife, who I will of course accompany

Believe me
Ever yours
James D. Dana

but I thought it best to get a reply to enquiries in order to satisfy the News Co. — He happened to have an article published in the Proc. Roy. Soc. which Prof Croll asked us to reprint all in type; and I concluded to withhold it and write Croll, putting my enquiries to him. And that is the way the case stands.
We cite occasionally from the Roy. Soc. Proc. Phil. Mag., Nature, & the Geol. Mag., Linn. Soc. It is quite recently that we had an article sent us by Lockyer for republication with the cuts. The one sent nos. back, by Huggins, was sent by Lockyer. — Wilson has written us nothing, showing that he has no intentions on the subject.

Yours D.D.

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I have an article of 20 pages in our Sept. no., and this may give me more close work than is good for me. The variety in the Misc. in our Sept. no. may not be increased by any selections of mine on account of it.

Office of The American Journal of Science and Arts,
JAMES D. & E. S DANA, PROPRIETORS.
New Haven, Ct., Aug 1880

Dear Gray:
Your notice for the
Boston no. has gone to the
Printer & you will have a proof
from. I should like for
Boy may I advertising the Journal
in Gardeners to get us, in the course of
a month or so, a short catalog
the titles of articles of the half
dozen preceding nos. of the Journal
headed by a brief note, and last
it to all names in the Naturalist
Directory. The brief note would ask
the recipient to favor it on
Already = Publication. —
Without can we do more?

the American paper in England, and gained it. This led the American News Co. to write us that if we reprinted papers from English publications they should strike the name of our Journal from their Foreign Advertising list. The recent decision making it in their opinion unsafe for any agent to sell such Journals, or the numbers so transgressing. — I wrote them that we should wait & make enquiries before making again such citations. I did not think that we, or our agents in England rather, would be in danger of prosecution for any such reprintings as we had hitherto made;

but have learned nothing in our subscription list this year, and I doubt if we will get back again much. — Advertising in newspapers does not seem to pay, & I think many things of the kind we have done do, & will not be done again by us.

With kind wishes

Yours truly

A. Sutherland

James B. Pond Esq.

You probably saw the notice in the paper of an American Journal having been refused admission to England because it contained an article reprinted from an English Journal. A similar decision has been given in the case of the Nineteenth Century, the English publishers having refused to submit to the demand of the agent to

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most agreeable to me, and to us, personally, and that is — adding your name to those of Editors, and if it would be as agreeable to you also, we would have it so on another volume — putting the names in the order you prefer. This, moreover, would give a Cambridge feature to the Journal beyond what it now has. Let me know how this strikes you. I appreciate fully your long labors in behalf of the Journal, and wish we could make it pecuniarily profitable to you. — Mrs intends to be at Cambridge at the meeting. But let me hear direct from you about this last matter.

Yours, as ever,

James D. Dana

Office of The American Journal of Science and Arts,

JAMES D. & E. S DANA, PROPRIETORS.

New Haven, Ct., ... Dec. 23 1880

My dear Gray:

My letter must [illegible] without answering that question about [illegible] Reading my mind to find [illegible] subscribers. We do, then, [illegible] our own. But we have no knowledge of the names to which Tuttle sends his copies. As that we cannot make any move further. We are glad to see the mind, [illegible], and would also say a word again my number that might fail, without change.

Fridum order us to send his packages through Hoffkin, Gifford & to [illegible] Boston steamer. His

has thought of it in connection with the reputation of the College, or has regarded it as having anything to do with Yale. Yale has done nothing for it, not given it a dollar or any way shown it favor. I had never thought of it as a Yale institution & have sought to make it otherwise. - I have now considerable invested in it having given Dillman $1500 for his half interest in it; and of course I hope that the Journal may continue to be a success scientifically if not so financially. There is one change which would be

made that I should like to have at once, through resignation in Boston, two members were recently elected who are officers of the Boston Club & I should like to have their names dropped from the list. Will you kindly do this for me? Dillman has paid his dues for 1894 & I have returned his receipt. We need all our exchanges by mail. Dillman's address is Boston Public Library. Please send me a list of members who have not paid their dues for 1894 & I will see that they are dropped from our lists unless they pay up at once.

Yours sincerely

Professor Newton, whom I suppose you will be glad to see in Cambridge, shows how different the feeling is in the two countries. Scientific Cambridge from what it is here. I do not think that there is any one who will be Newton who

New Haven, Nov. 13. 1891

My dear Gray:

You can see this soon announcing your safe return to your home put my pen in my hand to write a word of Congratulation. I wish I could do more, in other words, have a talk with you & your good belongings the past year. Your words "All well" mean I doubt not, your wife as well as yourself, and that is a pleasure to us here. My wife begs give my love to Mrs Gray, and the Doctor too.

I have nothing new to report of myself & household. College keeps me occupied. Mr Goodale very kindly sent the formal testimonial notice until that great calamity befell his little family. I sympathized with him profoundly for I knew of that sorrow by experience; and I would have written him had it not seemed like intrusion. We have also been much indebted to Prof. Farlow for contributing to the Herb. &c. — — With very kind regards to your wife, I am as ever

Sincerely Yours
James D. Dana.

reading of this note on the subject.

It would give us pleasure, my dear Gray, to have your name in the first line of editors, as proposed to you a year ago, if it is also agreeable to you.

Ever sincerely yours
James D. Dana

New Haven, Nov. 23. 1881.

My dear Gray:

It is a great pleasure to have more of your contributions for the Jour. Sci. — Whatever sent two days since is already in type & I believe a proof has gone to you. — Another lot came today, which I suppose you intended for our January no., as it is quite time our Dec. no. were out of the hands of the printers; and, in fact, we have, with your first sending omitted, been closed up the last page. —

The suggestion you make is ours too. But the difficulties are great. We have cited no whole articles from any foreign Journal the year past, because we have had no room; and we have had comparatively little Miscellany in some nos. for the same reason. Articles that have been accepted for a number have proved longer than expected; and some come late with a very urgent request for a place, and then we

Cannot refuse such requests from some contributors from a fear of losing their contributions. In these other ways, the Miscellany space (25 pp.) is marched upon. Our December no became filled up with articles omitting 13 pp. of the end in these ways, and an index has to take 6 of the 13. By overrunning to 84 pp. we have added somewhat to the room.

We have put in all the physics Cambridge has sent us - generally about 2 pages; and have wished it were more. - Baker has been about for the 4 mos. past, and thus we have not had the usual Chemical abstracts. Of Geology & Mineralogy we generally have on hand some pages of abstracts that are held over from numbers for want of space.

Nature has little but abstracts, and it has indefinite space; one of its pages will make 3 of ours.* And being a weekly it can command advertisements in large numbers We could not make more

space, with our present no. of pages; and could not add other pages without adding to the price - Phil. Mag's subscription price is 16 per. cent more than ours. - To give the Journal the form of Nature & print it in one Septuple would cheapen its production much, but it is hardly feasible or advisable. Is it not so?

For our January no. we have a paper by Loomis (meteorological) of 20 or 22 pages (probably); one, by LeConte, of 15 or 16; and one by B. A. Gould of 20 or 22, by Gould: Estimate - These papers make together 55 to 60 pages; and besides, we have already two short articles. I wrote Gould that we might have to lay his paper over to February. But it was unnecessary to do it, for we had no excuse except it was desirable to have greater variety in our January no. Loomis spoke first; & besides has paper half the papers of his plates, and something like $1000 a year for a clerk to help in preparing them - for he does little else.

You thus will appreciate some of our difficulties - They are greater in the reality that will appear in the

* Nature often runs 3 of its pages to a single section on abstract; which is in our form - 5 or 6 pages; or a third of the Miscellany (25 pp.).

New Haven, Dec. 4. 1881.

My dear Gray:

I thank you warmly for your kind letter. If we cannot have your name advanced, it is a great pleasure, and a great source of encouragement to know that we shall have still, as we have long had, your invaluable aid. You write as if you had not your usual good health. I had my check some years ago: And yet you see I keep at it. I walked 7 or 8 miles at Middletown yesterday, studying terraces with a level

– my fifth examination there recently. I wish you & Mrs Gray the best of health for a long time to come.

As ever Sincerely Yours
James D. Dana

New Haven, Feb. 17. 84

My dear Gray

I was at Sumner's funeral — and had it in me that he has gone. — Another death, which has just occurred, touches me more nearly still — that of Wells Williams — who died at 9 last evening. He had recovered to a considerable degree from the disability occasioned by a slight paralytic stroke, and until within a month had taken his usual walks. But during that month there has been a gradual loss of strength

and you will have proof of them soon. It is possible that we may want to defer one of the notices to another number, as articles are crowding on the Miscellany.

I was sorry to learn through your note of the death of Engelmann.

Sincerely yours

James B. Dana

and yet he was able towards about his room in Trinity and had his usual class until Thursday night. We began our A. B. Exercises in the same little school and it has been a great pleasure to me often how wonderfully talented the boys are, and how human we found a common city, and our names in the same College catalogue.

Mrs. Dana. I have come, together with the pamphlet containing Carpenter's monograph

Office of The American Journal of Science and Arts.

JAMES D. & E. S. DANA, PROPRIETORS.

New Haven, Ct., Feb 11 1886

My dear Gray

I return the
two pp. of Ms. — Thanks
for the Botanical Notices.
It is possible we may
to defer to another
number no V.

Your notice of
Agassiz was a very
pleasant one — I showed
that you understood him.

Faithfully Yours
James D. Dana

New Haven, Oct. 26 1886

My dear Gray;

You have conquered me by your explanations and your very kind words. — Only last night, I got but three hours sleep ~~having~~ to a little exciting cause yesterday. Still if I may retreat so easily, my objection is diminished greatly, and I will write to President Eliot withdrawing my letter.

It was hard to decline; ~~And~~ all the more ~~that~~ you were on the ground — my best—

of friends. You should have a like honor.

With the kindest regards to Mrs Gray.

Faithfully yours
James D. Dana

any particular item required?

Dear Dr Gray

You have won — you see — I thank you heartily for all your kind words, they touched us both deeply. It would be very pleasant to be with you, and so it will be with the Cooked.

My kindest greetings to Mrs Gray.
Ever yours cordially
J. D. Dana.

New Haven, Nov. 14. '86

My dear Gray:

I have your note and will erase the name of the Intelligent Journal as you suggest.

I find myself in quite good condition after my Cambridge visit — much better than I dared to hope. The celebration was excellent & grand in all respects, and I enjoyed it far beyond my expectation. Cambridge is a year ahead of us in means & equipment, and success to it I say —

With my kind regard
to Mrs Gray, to whom Mrs
D. sends her love —

As ever
Cordially Yours
James D. Dana.